The Effect of Labor Market Tightness on Recruiting Levers: Evidence from US Employers during Covid-19*

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Abstract

How do employers' recruitment strategies adapt to labor shortages? This paper estimates the response of employers' posted wages and skill demand to labor market tightness. Leveraging Covid-19's heterogeneous impact on labor demand across industries and detailed online postings data, we propose a precise measure of local tightness whose variation relies on a shift-share IV. We find that tightness decreased the likelihood of employers listing education and experience requirements, but increased required years, when listed. Controlling for composition, these findings are statistically significant for low-wage, low-skill positions, where a lower bound constrains years required. We document complementarity in recruitment levers, as tightness also significantly raised advertised salaries for these positions, contributing to the reduction in wage inequality in the post-pandemic US.

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1 Introduction

How do employers' recruitment strategies adapt to labor shortages? Do they relax their skill requirements to draw from a potentially larger pool of candidates, or do they raise wages to price out competitors? And to what extent do these potentially complementary levers help explain recent labor market trends? Labor market tightness ebbs and flows with the business cycle boom and bust, and this cyclicality makes it difficult to study the effect of a change in labor market tightness on employers' behavior due to concurrent economic changes. However, the *unexpected* nature of the Covid-19 pandemic in 2020, as well as its marked *heterogeneity* across industries, offer the opportunity to make progress on this challenge and deepen our understanding of the role of tightness in post-pandemic labor markets.

This paper leverages pre-pandemic variation in industrial composition across US commuting zones (CZs) and Covid-19 heterogeneous impact across industries to estimate the effect of (Covid-induced) labor market tightness on employers' skill demand and wages. We rely on the fact that, for instance, sectors like *Health Care and Social Assistance* faced a shortage of workers at the onset of the pandemic, whereas others like *Arts, Entertainment, and Recreation* had a surplus of workers. The *Accommodation and Food Services* sector saw a sudden decline in demand with the introduction of stay-at-home orders, followed by a rapid surge in demand as well as labor shortages due to workers resigning and reallocating away from those jobs. Meanwhile, *Professional Services* and *Management* jobs in tech and finance experienced much milder swings with the widespread expansion of remote work. As a result, the heterogeneous effect of the pandemic across industries organically created variation in labor market tightness across local labor markets with different industrial composition.

Ours is an important question both because there are compelling reasons to believe that tight labor markets will persist well beyond pandemic years, and because there is theoretical ambiguity about employers' response to it. The restriction of global mobility during the height of the pandemic, and for a while thereafter, has led to significant migrant shortages in US markets. Similarly, the onset of the pandemic caused labor force participation to plummet by as much as 3 percentage points by catalyzing the anticipated retirement of the baby boomers. Well-documented declines in fertility rates and native workers internal mobility further exacerbated employers' difficulty in filling openings. Collectively, these trends point toward persistently higher levels of labor markets tightness beyond the pandemic, which we indeed observe. Theoretically, faced with tight labor markets, employers can adopt one of two recruitment strategies. One strategy is to adjust and lower their hiring standards. For example, an opening that would normally require someone with five years of experience might now accept applicants with three years of experience. Another strategy is to improve job compensation and benefits to attract the limited number of qualified workers in the market. This could take the form of higher wages, health insurance, and/or providing more flexible work options such as hybrid or remote work.[1]

[1]While we appreciate that non-wage amenities are an important component of compensation packages and some

This paper empirically tests both hypotheses, and examines whether the impact was different for a group of low-wage and low-skill jobs particularly affected by tightness in the post-pandemic US labor market. To observe firms' hiring behavior, we use online job postings data from Lightcast, a database collected from roughly 50,000 websites covering the near universe of online job postings for all metropolitan statistical areas (MSAs) in the US, which we have available from 2018 through 2022. These job ads in turn contain information on employer's education and experience requirements as well as offered wages. We construct extensive and intensive margin measures for all three outcomes at the occupation-commuting zone-year level. The extensive margin measure captures the share of postings that list a requirement, such as an education requirement, whereas the intensive margin measure captures the level required, in this example the number of years of education, for postings that specify an education requirement.

We complement these online job postings data with Local Area Unemployment Statistics (LAUS) data to build a measure of labor market tightness. In particular, thanks to the granularity of information in online job postings, we propose a measure of tightness that can be constructed at a much more local level than measures in earlier studies and that does not rely on unemployment statistics, known to be very imprecise during the pandemic.[2] We then construct a shift-share instrument by interacting nation-wide growth in online job postings for each 3-digit NAICS industry with pre-pandemic local industry shares from the American Community Survey (ACS) 5-year file for 2015-2019, which allows us to isolate exogenous variation in our measure of local tightness.

Our analysis identifies and zooms in on low-wage, low-skill workers, whom we define as workers in the bottom tercile of the wage distribution in 2015-2019 ACS who work in low-service and blue-collar jobs, as defined by Forsythe, Kahn, Lange, and Wiczer (2022).[3] Crucially, in 2019, job postings for these positions featured much fewer years of education and experience, constraining employers who wished to lower the skill level required, thus establishing a lower bound. We are therefore interested in understanding whether this downward constraint to a single recruiting lever might have driven employers to act on the other, contributing to the recent decline in US wage inequality.

Two facts suggest that this dynamic might be at play. First, relative real wages (10^{th}-to-90^{th} percentile and 33^{rd}-to-90^{th} percentile) increased markedly (by 8 and 4 percentage points, respectively) between the onset of the pandemic and 2022, driven by increases at the bottom of the wage distribution (Figure 1), as also documented in Autor, Dube, and McGrew (2023). Second, as we will show in Section 4, low-wage workers tended to be employed in industries that experienced large demand swings during the pandemic. Industries like *Accommodation and Food Services* and *Arts, Entertainment and Recreation* suffered significant employment declines early on, while *Transportation and Warehousing*, and *Retail Trade* drove the rebound and spikes in job openings throughout 2021

workers highly value them, in this paper we focus on advertised wages only.

[2]We elaborate further on these advantages in Section 5 and Appendix A.

[3]Defined as occupations with the following SOC: 33, 35 to 39, 45 to 53 and 412.

and 2022 (Figure 2). How, then, did recruiting difficulties, highly heterogeneous across industries and geographies during these pandemic years, translate to wage dynamics?

While the sudden onset of the pandemic provides an appealing setting by helping to avoid biases from anticipation effects and reverse causality, it also raises concerns, as Covid partially altered working conditions and hiring processes. To strengthen the credibility of our design, we conduct several validity exercises and impose some restrictions to ensure we appropriately address the complexity of the shock. First, we run a full set of diagnostics for shift-share IVs following Goldsmith-Pinkham, Sorkin, and Swift (2020). We show that the variation our instrument exploits is primarily driven by industries where firms likely faced hiring difficulties due to sudden demand surges or collapses during and after the pandemic, supporting our interpretation that labor shortages, rather than other simultaneous changes, were the main shock affecting skill demand in these sectors that we emphasize when motivating our empirical strategy. Second, to address changes in work conditions and deviations from typical hiring processes that may mechanically lead to a change in our outcomes, we leverage the richness of our data and restrict the analysis sample to in-person job openings only.[4] Third, we validate our novel measure of labor market tightness by comparing it with the one used in Autor et al. (2023), which relies on different data and construction methods, finding broadly consistent results. Finally, to address concerns about endogenous compositional changes in employers, we then restrict our data to incumbent employers and re-run our main analysis studying changes in outcomes within firm-occupation pairs, thereby also absorbing many local confounders related to the multifaceted nature of the pandemic.

We identify three main findings. First, our 2SLS estimates show that higher local labor market tightness had a negative effect on the share of job ads listing an education or experience requirement. In other words, employers facing hiring difficulties primarily resorted to removing skill requirements from their job postings. Once we include firm-by-occupation fixed effects to control for compositional changes in our most restrictive specification, the effect for education remains negative and statistically significant for low-wage, low-skill jobs only. An increase in tightness relative to 2019 that would bring a commuting zone in a given year from the 25$^{\text{th}}$ percentile to the 75$^{\text{th}}$ percentile in the distribution of observed changes is predicted to decrease the share of job postings requiring some level of education by roughly 0.04 percentage points for low-wage, low-skill positions.

Second, in terms of level of skills required, we find that tightness did not significantly increase average years of education and experience listed, except for years of education for low-wage, low-skill jobs. This result on the intensive margin of education is confirmed when we focus on changes within firm-occupation pairs, although the coefficient is significant only at the 10% level. Doubling CZ's tightness relative to 2019 is predicted to increase average years of education listed by 0.01 years in postings for low-wage, low-skill positions. Assuming that postings without a listed education

[4] We also add local controls for remote-work propensity to account for remaining confounders related to the online reallocation of certain tasks within in-person jobs.

level or experience range do not require any, our interpretation of these results is that tighter labor markets led employers to completely remove their requirements for some low-wage, low-skill jobs to overcome a lower-bound constraint in level of skills, while raising requirements where they needed management or supervision staff to oversee less trained or under-skilled workers. We provide evidence of this mechanism by showing that, for low-wage, low-skill jobs, tightness had a positive and statistically significant effect on the likelihood that postings require management skills.

Third, while our estimates do not point to strong evidence that tighter labor markets increased the share of postings listing salaries, we find that tightness prompted employers to increase the salaries advertised, when listed. An increase in local labor market tightness by 10 log points (roughly 10%) increases salaries advertised for low-wage, low-skill jobs by 1.4% relative to 2019, which is a remarkable effect given the +4% average change in the period. Crucially, this finding is only positive and statistically significant for low-wage, low-skill jobs. Overall, our findings suggest that employers rely on multiple levers to attract new candidates when filling vacancies becomes more challenging, including adjusting human capital requirements and offering higher wages. We rationalize these results through the lens of a search and match model that we describe in the next section.

This paper relates to three strands of the literature. First, our work contributes to a large literature on the effect of the Covid-19 pandemic on labor markets. Thus far, the literature has explored many outcomes such as changing labor force composition (Albanesi and Kim, 2021), wage compression (Larrimore, Mortenson, and Splinter, 2023), native mobility (Peri and Zaiour, 2023) and reallocation (Barrero, Bloom, Davis, and Meyer, 2021). Most closely related to this project is work by Gu and Zhong (2023) and Forsythe et al. (2022) who also use Lightcast online job openings data to examine the effect of the pandemic on posted skills. Using spatial variation in duration of stay-at-home-orders, Gu and Zhong (2023) find suggestive evidence that longer stay-at-home orders shifted demand from "people-oriented" to "operation-oriented" management. Focusing on low-skill occupations, Forsythe et al. (2022) document the evolution of skills requirements at the national level across different waves of Covid-19. They observe that during the pandemic employers were more likely to list education and experience requirements, but conditional on listing, they required fewer years of education and experience.

Our paper differs from these two cited papers in three ways. First, we isolate the role of tightness to explain changes in skills, rather than using Covid-related policies or time indicators that capture Covid-19 waves as explanatory variables. Second, our empirical approach based on a shift-share instrument allows us to recover a casual estimate by using variation in industrial composition across commuting zones and Covid-19 differential impact on labor market tightness across industries. This contrasts with the state variation in labor market tightness used by Autor et al. (2023) and the focus on the evolution of skills for the US market as a whole discussed in Forsythe et al. (2022).[5] Third,

[5]In Appendix C we discuss in detail how to reconcile our results with Forsythe et al. (2022)'s findings.

this paper connects research documenting Covid-19 effect on wages (e.g. Autor et al. (2023)) and skill demand (e.g. Forsythe et al. (2022)) by bringing these two outcomes into the same framework to evaluate their complementarity as recruiting margins employers leverage.

Second, this paper is closely related to the budding literature on skill demand cyclicality. Studying the effect of slack labor markets following the Great Recession, Hershbein and Kahn (2018) and Modestino, Shoag, and Ballance (2020) find that employers persistently increase their skill requirements, i.e., upskill. On the flip side, faced with tight labor markets during the fracking boom, Modestino, Shoag, and Ballance (2016) find that employers reduce their education and experience requirements, i.e., downskill.[6] Our paper expands this literature by studying the effect of a labor demand –not labor supply– shock on employers' hiring strategy. It is also the first study to evaluate the persistence in upskilling after a boom fades.

Third, our paper contributes to the labor market tightness literature, which studies the effect of labor market tightness on different outcomes like wage growth (Autor et al., 2023) and union activity (Pezold, Jäger, and Nüss, 2023). Our paper contributes to this literature by introducing and validating a more granular measure of labor market tightness. In the absence of Lightcast online job postings data, prior work has relied on using the Job Openings and Labor Turnover Survey (JOLTS), the primary government source on US job openings, to construct a measure of labor market tightness. However, JOLTS is publicly available only at aggregate levels, such as 2-digit NAICS industries or states (Peri and Zaiour, 2023). Using Lightcast online job postings, we proxy for the increase in vacancies using growth in number of online job postings for each industry and construct our labor market tightness as a ratio of online job postings to postings plus employment. This novel approach circumvents measurement error concerns surrounding temporary lay-offs and pandemic headline unemployment numbers.[7]

The remainder of this paper proceeds as follows. Section 2 outlines the theoretical framework intended as a guide for our main empirical exercise. Section 3 and Section 4 introduce the data and describe the characteristics of postings and workers in the low-wage, low-skill jobs respectively. In Section 5 we present our methodology, discussing the empirical framework, our measure of tightness and the shift-share IV approach. Section 6 presents our main results. Finally, Section 7 concludes.

2 Search and Match Model

In any period, an ordinary labor market will have a number of unemployed workers looking for jobs and a number of firms with open positions, yet some vacancies remain unfilled. In this section, we introduce a standard search and match model specified in continuous time that accounts for search

[6] Modestino et al. (2016) use change in local unemployment as their right hand side variable to capture tightening labor markets.

[7] We expand this discussion on methods in Section 5 and Appendix A.

frictions.[8] In this framework, firms and workers negotiate over wages that are a function of labor market tightness, θ, the ratio of vacancies to unemployed workers at time t (such that $\theta_t = \frac{V_t}{U_t}$). In theoretical work, researchers often calibrate the model to find a pair of θ and w that satisfy negotiation conditions. In this paper, we aim to derive the model's predictions about the effect of labor market tightness on wage growth for different skill levels, then test them empirically in Section 6.

Firms and workers negotiate over wages in an attempt to increase their respective shares of the total surplus. A firm surplus is the difference between the value of a filled position and a vacancy. The value of a filled position equals net profit, $p - w$, where p is real output and w is the cost of labor (wages), and for any given position, the firm faces probability s that the job becomes vacant again due to regular turnover. Therefore, with r as the interest rate for discounting time, the present-discounted value of a filled position is:

$$rJ = p - w + s(V - J) \tag{1}$$

On the other hand, the value of a vacant position is the expected value of filling the position (gaining J), which occurs with probability $q(\theta)$, minus search cost, pc.[9] Notice that hiring cost, c, is proportional to productivity, p, to capture the fact that hiring a skilled worker requires an equally or more skilled worker to evaluate the skill of potential candidates.[10] This means that the present-discounted value of a vacant position is:

$$rV = q(\theta)(J - V) - pc \tag{2}$$

Assuming that firms can freely enter the labor market, then the value of a vacancy will equal zero in equilibrium. Applying this condition to equations 1 and 2 gives us the following job creation condition:

$$J = \frac{pc}{q(\theta)} = \frac{p - w}{s + r} \tag{3}$$

where a job will exist as long as the benefit of filling the position ($\frac{p-w}{s+r}$) outweighs the search cost, ($\frac{pc}{q(\theta)}$).

We next turn our attention to workers' surplus and value functions. A worker surplus is the difference between the value of employment to unemployment, where the value of being employed comes from wages taking into account the probability of separation and returning to unemployment.

[8] These models are often referred to as Diamond-Mortensen-Pissarides (DMP) models, which are the most common theoretical framework to analyze unemployment, wage formation and job vacancies in labor markets.

[9] A firm's job filling rate depends on the ratio of vacancies to unemployed workers in the market, hence q is a function of θ.

[10] In practice, search cost is probably a function of a constant cost, c_0, and a proportional cost, cp. A constant cost could be something like the cost of posting a job ad for a week on a job board. Since this cost is common across all types of jobs considered here, we assume it away.

More specifically, the present-discounted value of employment is:

$$rE = w + s(U - E) \Leftrightarrow E = \frac{w + sU}{r + s} \tag{4}$$

Similarly, the value derived from unemployment comes from unemployment benefit, z, and the probability of future employment (gaining E), which occurs with probability $\theta q(\theta)$. Therefore, the present-discounted value of unemployment is:

$$rU = z + \theta q(\theta)(E - U) \tag{5}$$

In equilibrium, filled jobs yield a higher return than the expected returns of a searching firm and worker. The division of these returns –economic rent– are then negotiated using Nash bargaining, following Pissarides (2000). A Nash solution to the bargaining problem assumes that wages are renegotiated whenever new information arises. Firms and workers negotiate over their respective private surplus, such that for a given wage rate, w_i, a firm's expected return from job J_i becomes: $rJ_i = p_i - w_i - sJ_i$ and a worker's expected return from job J_i is: $rE_i = w_i + s(U - E_i)$. Therefore the Nash solution is the w_i that maximizes the worker and firm's net gain.

$$w_i = argmax(E_i - U)^{\alpha}(J_i - V)^{(1-\alpha)} \tag{6}$$

where $\alpha \epsilon (0, 1)$ captures the bargaining power of workers and hence their share of the rent. Taking the first derivative with respect to w_i gives us:

$$(1 - \alpha)(E_i - U) = \alpha(J_i - V) \tag{7}$$

Using equations 3 and 4 to plug in for J_i and E_i respectively, we obtain:

$$w_i = \alpha p_i + (1 - \alpha)rU \tag{8}$$

We then use equations 3 and 6 to derive an expression for rU, and plug it into equation 8. This gives us the following wage setting rule:

$$w_i = (1 - \alpha)z + \alpha(p_i c\theta + p_i) \tag{9}$$

$$\Leftrightarrow w_i = (1 - \alpha)z + \alpha p_i(c\theta + 1) \tag{10}$$

Equation 10 implies that wages are increasing in unemployment benefit (z), labor market tightness (θ) and productivity (p_i). These findings are consistent with rational behavior, where higher z leads to higher reservation wage, higher θ captures that job offers arrive faster to workers than workers to vacant positions, and higher p_i implies a more productive worker. Since we are interested in wage growth rate, taking the natural logarithm of equation 10 we get:

$$lnw_i = ln[(1-\alpha)z_i + \alpha p_i(c\theta + 1)] \tag{11}$$

To better understand the effect of labor market tightness on wage growth, we then take the derivative of equation 11 with respect to θ and obtain that:

$$\frac{\delta lnw_i}{\delta ln\theta} = \frac{\delta lnw_i}{\delta\theta}\theta \tag{12}$$

$$= \frac{c\theta}{c\theta + 1 + \frac{(1-\alpha)z}{\alpha p_i}} \tag{13}$$

Since $\theta\epsilon(0,1)$, we can conclude from equation 13 that wage growth is increasing in labor market tightness. In other words, the model predicts that as labor markets become tighter, and we have fewer workers per vacancy, wage growth increases. But how does this growth vary across skill groups? We can model this heterogeneity in two ways, one is to vary θ by skill group such that each group of occupations o face a different labor market tightness, i.e., θ_o. Another approach is to allow α to vary across skill groups. While prior to the pandemic low-skill workers had limited bargaining power, labor shortages during the pandemic increased their leverage such that at the limit $\alpha_{low-skill}$ converges to $\alpha_{high-skill}$. Next, we empirically test the theoretical model using Covid-driven variation in labor market tightness (θ) across commuting zones and estimate its effect on wage growth by skill level.

3 Data

In this paper, we define local labor markets at the commuting zone (CZ) level and leverage variation in industrial composition across commuting zones to study changes in employers' hiring strategy. To answer our question, we rely on various data sources. For our outcome variables and the shift component of our shift-share instrument, we use online job postings data from Lightcast. For our explanatory variables and to build the share component of our shift-share instrument, we use the American Community Survey (ACS) and the Local Area Unemployment Statistics (LAUS) data.

3.1 Job Postings Overview

The key data for observing employers' posted skills and wages comes from Lightcast's online job postings data, previously circulated as Burning Glass Technologies. Lightcast (LC) is a data analytics company that scrapes roughly 50,000 websites, including job boards and company pages to build a dataset of the near-universe of online job postings from 2010 to 2022 for all MSAs in the United States.[11] Their algorithm identifies newly posted job ads, removes duplicates, and standardizes common information across postings. For each job posting, we have information on

[11] Job postings are available at the establishment level, or the specific physical branch of a firm.

education level, field of study and experience requirements as well as an average of nine skills extracted from the posting's open-text. We also have advertised wages for approximately 30% of all postings. This breadth and detail of LC's vacancy data makes it uniquely suited to help us unpack the black box of firm demand margin within and across occupations and industries.

Using Lightcast job openings data offers two advantages over using the Job Openings and Labor Turnover Survey (JOLTS), the primary government source on US job openings. First, Lightcast is available at a more granular industrial and geographical level, which is essential to our identification strategy since our "treatment" happens at the commuting zone level. JOLTS data is the product of surveying a nationally representative sample of 21,000 US business establishments across all non-agricultural industries in the public and private sectors for all 50 States and the District of Columbia. However, vacancies are typically only available at aggregate levels (dis-aggregated by states or by 2-digit NAICS industries) whereas LC provides vacancy-level data at location (city and county) and 6-digit NAICS. Second, Lightcast data has a richer set of job posting characteristics, which allows us to examine various margins of skill demand that are otherwise difficult to observe.

Unfortunately, as a by-product of relying on online job ads to capture job openings, LC over-represents white collar jobs (Hershbein and Kahn, 2018; Babina, Fedyk, He, and Hodson, 2020). This white-collar bias does not pose a serious threat to our results for two reasons. First, as of 2020, high-skilled jobs make up 60% of the entire US workforce such that, even in the worst (and, as we later discuss, unfounded) scenario of severe bias, our findings will help us understand hiring demands for a significant share of the labor force.[12] Second, and most importantly, a growing share of job ads have moved online over time, including those for low-skill jobs, such that recent validation exercises by Chetty, Friedman, Stepner et al. (2020) and Dalton, Kahn, and Mueller (2020) find that Lightcast data is well-aligned with JOLTS numbers for the private sector.[13]

Our main data sample is the subset of postings with populated location (county), industry and occupation from 2018 to 2022 located in the continental US and excluding farming, military and public administration openings. Using this sample, Figure 3 shows the monthly deviation in the number of postings relative to the average across months in 2019. After an initial drop at the beginning of the pandemic, the number of job postings consistently rose above the pre-pandemic monthly average, reaching the highest level in the first half of 2022, with a peak of over 1.2 million job ads in excess of the 2019 average. As one might expect, the share of job postings with remote or hybrid positions increased after the onset of the pandemic. We document this phenomenon by tracking both the in-person postings only (solid blue line) and the remote or hybrid job postings added to the in-person only postings (lighter dashed line). In order to avoid confounding effects coming from skill changes due to remote work, we further restrict our main analysis sample to

[12] Source: The Department for Professional Employees' 2021 Fact Sheet, which relies on data derived from US Census Bureau and Bureau of Labor Statistics.

[13] Other validation exercises conducted and made available by Lightcast provide additional reassuring evidence, both for the time series and in geographic terms (see Figure 15 in Appendix C).

in-person postings only. This leaves us with a total of about 162 million postings.

3.2 Human Capital

For each outcome, we define an extensive and an intensive margin measure to capture within-occupation changes in skill requirements, in line with definitions by Hershbein and Kahn (2018). First, to measure the *extensive* margin, we compute the fraction of job postings that require skill s for occupation o in commuting zone c at time t. Second, to measure the *intensive* margin, we compute the average level of skill s for occupation o in commuting zone c at time t, focusing only on postings that list skill of interest. For example, when looking at the effect of labor market tightness on education requirement, our extensive margin variable will capture the share of postings in commuting zone c at time t that list an education requirement for a given occupation o. Conversely, our intensive margin variable will capture the change in average years of education listed, say, from 14 years (Associate's degree) to 16 years (Bachelor's) for postings that list an education level. Our main outcomes of interest are the extensive and intensive margin variables of education, experience and posted wages.

In Figure 4, we plot the national change in the extensive and intensive margins of education and experience relative to the 2019 average, respectively. For education, we observe a sudden 4 percentage point drop in the share of postings listing education at the beginning of the pandemic, which bounces back in 2021 and then declines again in 2022. On the intensive margin, conditional on listing education, employers required between 0.4 and 0.5 years less education at the beginning of pandemic, but the average years required started to increase again, re-gaining more than half of the decline observed by the end of 2022. For experience, we observe a drop in the share of postings listing experience at the beginning of the pandemic, which persists until the end of 2021, but by the end of 2022 the share of employers including an experience requirement is up by 2 percentage points relative to 2019 average. Yet, as we see from Panel B, conditional on requiring experience, employers tend to list a lower number of years. This decline is persistent, with the average level of years required (red solid line) in December 2022 remaining close to the lowest point reached in mid-2020.

In Figure 5, we show the change in average years of education and experience required between 2019 and 2022 by commuting zone. From Panel A, we observe that some commuting zones experienced an increase in the average years of education well above 1 year (darkest blue areas in Panel A), while others reported a decrease well below 1 year (darkest red areas). And in line with the evidence from the national time series, Panel B shows that the majority of commuting zones experienced a decline in listed experience with all commuting zones in states like California, Florida and Virginia reporting drops in average years of experience.

In Figure 6, we report the evolution of the average share of postings advertising a salary (black solid line, y-axis on the left) and of the average level of salaries after controlling for inflation (gray solid line, y-axis on the right).[14] The share of job postings containing salary information doubled by the end of 2022, increasing by roughly 20 percentage points relative to the average across 2019 month. The average real advertised salary, on the other hand, decreased markedly at the beginning of the pandemic but recovered over time, returning to around its 2019 average by the end of 2022.[15]

3.3 Employment, Shares and Controls: ACS and LAUS

For our analysis, the American Community Survey 2015-2019 five-year sample plays three roles. First, we use ACS data to compute pre-pandemic 3-digit industry employment shares for each CZ. Second, we use it to identify occupations of the most vulnerable group in the labor market, the low-wage, low-skill workers. Following Autor et al. (2023), we define low-wage workers as workers in the bottom tercile of the ACS wage distribution.[16] Following Forsythe et al. (2022), we then define low-skill workers as workers in low-skill service (SOC 35 to 39 and 412) and blue collar (SOC 33 and 45 to 53) jobs. Creating a list of occupations that satisfy these two definitions in ACS, we identify them in Lightcast data and use them to split our sample into "low-wage" and other. This procedure allows us to study the effect of local labor market tightness on the most vulnerable group of workers. Third, we use ACS data to control for heterogeneous demographic composition across commuting zones, namely share of: foreign-born, women in the labor force, manufacturing employment, college-educated workers and remote employment probability using Dingel and Neiman (2020)'s methodology.[17]

Finally, our main explanatory variable, labor market tightness, is built as the ratio between vacancies and total jobs available (i.e., the sum of vacancies and filled jobs). To that end, we track vacancies using Lightcast online job postings, and county-level employment using the Local Area Unemployment Statistics (LAUS) data.[18] Section 5 provides a more detailed description of this variable.

4 Low-wage, Low-skill Jobs

Evidence from the literature suggests that skill demand changed significantly for low-skill service jobs (Forsythe, Kahn, Lange, and Wiczer, 2022), and that historically vulnerable workers, such as young non-college workers, experienced a relatively rapid wage growth due to labor market tightness in the wake of the pandemic (Autor, Dube, and McGrew, 2023). Therefore, in our paper, we examine whether employers pursued a different recruitment strategy for this subgroup of workers

[14] For this figure and the main analysis, Lightcast salaries have been converted to December 2019 USD dollar using CPI data from the BLS (Bureau of Labor Statistics).

[15] We are not controlling for compositional changes in the pool of vacancies.

[16] we compute terciles of the distribution by converting wages to 2017 US dollars, consistent with our use of the 2015-2019 ACS data. Autor et al. (2023) use monthly CPS data to compute the 2019 wage distribution and isolate low-wage occupations whose workers' dynamics are then examined. Very little would change as long as this exercise is conducted before the pandemic.

[17] Estimates are publicly available at: `https://github.com/jdingel/DingelNeiman-workathome`.

[18] Since our geographical unit of analysis is commuting zones (CZs), we aggregate county and PUMA level data from ACS, Lightcast and LAUS to CZs using Autor and Dorn (2013)'s crosswalk.

than the rest of the market. In this section, we first characterize low-wage, low-skill jobs and then present education, experience and wages trends separating out openings for low-wage, low-skill from other openings in the market.

Characterizing low-wage, low-skill jobs, Figure 7 shows the industry (3-digit NAICS) and occupation (3-digit SOC) composition of online job postings in 2019 for this group. Panel A shows that job ads for these occupations posted prior to the pandemic belonged mostly to accommodation and food services (over 21% combined), passenger transportation (20%), administrative and support services (15%), and health care and social assistance (over 6%). In terms of occupations, Panel B shows that 43% of postings are food-related jobs (cooking, serving, preparing or processing food), 13% are cleaning-related jobs (janitors, cleaners, landscapers, etc.) and slightly more than 20% are transportation-related openings (freight, stock, and material hand-movers, truck and tractor operators, truck and tractor operators, etc.).

In Figure 8, we compare skills and wages of low-wage, low-skill workers relative to the rest of the labor force in ACS. Panel A highlights some important differences in terms of education attainment and labor market experience. Almost two-thirds of workers employed between 2015 and 2019 in low-wage, low-skill occupations do not have any college education (around 20% have no high school degree and around 40% have a high school degree as their highest degree completed), in contrast to less than one-third of workers in all other occupations. Workers in low-wage, low-skill occupations also tend to have less experience. Over 15% of them have less than 5 years of experience, while more than 30% have less than 10.[19] In Panel B, we see that low-wage, low-skill workers earn substantially lower wages than all other workers observed in the ACS.

Looking at this subgroup in Lightcast data, Figure 9 shows that demand for low-wage, low-skill service occupations increased significantly more than for all other jobs despite following the same trend. Turning to skill demand, Figure 10 shows the national evolution of skill requirements by group. Panel A shows the visually different dynamics of the extensive margin of listed education and experience across the two groups. At the onset of the pandemic, employers recruiting low-wage, low-skill jobs were more likely to list an education and experience requirement (up by between 6% and 8% percentage points compared to 2019), but while the extensive margin for experience remained well above the pre-pandemic average (dashed red line), education declined rapidly since the start of 2021, falling below 2019 average (solid red line). Conversely, for all other jobs, the share of posting listing education/experience declined slightly to below 2019 averages for the last three quarters of 2020, and then remained around the pre-pandemic averages (blue lines).

Looking at the intensive margin in Panel B, we see that the required years of both education and experience were back to the pre-Covid averages by the end of 2022 for low-wage, low-skill jobs, whereas years required for other jobs were still below pre-Covid averages. Crucially, baseline levels

[19] We impute labor market experience by subtracting education attainment to individual age, and we then aggregate workers into 5-year bins, starting from 1-5 years of experience up to 35-40 years.

must be taken into account here. Average years of required education and experience for low-wage, low-skill jobs in 2019 were 12.4 and 1.6, respectively. This suggests the presence of a lower bound in skill demand for these jobs due to limited scope for downskilling, as asking for less than 12 years of education would imply asking for less than a high school degree, while asking for less than one year of experience would practically mean accepting workers with no experience. It might well be that employers choose not to post any requirement when the requirement would amount to 0.

Figure 11 completes the picture by showing that posted salaries also reacted differently across groups over time. Specifically, while employers have become more likely to advertise a salary for both groups compared to pre-Covid (Panel A), the average salary level in real terms increased for low-wage, low-skill jobs, especially in 2021 and 2022 (Panel B). At the end of 2021, average real salary advertised increased by 5% for these occupations relative to before the pandemic. This evidence from postings data supports Autor et al. (2023)'s findings that the pandemic labor market tightness was correlated with wage compression. We now aim to recover the casual estimate of this finding.

5 Methodology

We use on a shift-share instrumental variable (IV) approach that leverages the interaction of Covid-19 heterogeneous effect on labor demand across industries with pre-pandemic variation in industrial composition across commuting zones. The intuition is as follows. Orlando and surrounding Orange county (FL) area have a high share of employment in the leisure and hospitality industry, with facilities like Universal Studios and Disney World. Therefore, they initially experienced slack labor markets followed by a rapid surge in tightness since demand dropped for leisure workers at the onset of the pandemic then rebounded when stay-at-home orders lifted. In contrast, the San Jose-Sunnyvale-Santa Clara area (CA) experienced a much milder swings in employment and demand given the area's high share of employment in the tech industry, where well-paid highly-educated workers benefited from the rapid rise of remote work arrangements. The sudden and unexpected nature of the pandemic meant that neither location had the chance to change their industrial composition ex-ante, and thus experienced very different levels of labor market tightness during and after the pandemic. We use this variation to answer the question: how does local labor market tightness affect employers' posted skills and wages especially for low-wage, low skill jobs?

To answer this question, we define local labor markets as commuting zones and look at within-occupation outcomes to account for variation in posted wages and skills across occupations.[20] Thus, we define our outcome variables as change in outcome y in occupation o in commuting zone c in year t relative to 2019, the last year before the pandemic. Running a standard OLS model, we have

[20]Commuting zones by construction are clusters of counties that are characterized by strong within-cluster and weak between-cluster commuting ties and are often used in the literature to think about local labor markets. This definition accounts for cities that span several counties and include non-metropolitan areas in our analysis.

the following specification:

$$y_{o,c,t} - y_{o,c,2019} = \alpha + \beta_1 \Delta^{t-2019}\text{Tightness}_{c,t} * S_o + \beta_2 \Delta^{t-2019}\text{Tightness}_{c,t} * (1 - S_o) \\ + \boldsymbol{X}_{c,2019} + \rho_r + \phi_t + \psi_o + \epsilon_{o,c,t} \tag{14}$$

where $\Delta^{t-2019}\text{Tightness}_{ct}$ is a measure of change in local labor market tightness in commuting zone c relative to 2019, S_o is an indicator for low-wage, low-skill occupations identified in Section 4 and $(1-S_o)$ is an indicator for all other occupations. Our coefficients of interest, β_1 and β_2, capture how a percentage change in tightness in commuting zone c in year t changes our outcome y relative to 2019 by group. $\boldsymbol{X}_{c,2019}$ is a set of variables to control for heterogeneous demographic composition across CZs and includes 2019 share of foreign-born, women in the labor force, manufacturing employment, college-educated workers and remote employment probability using Dingel and Neiman (2020)'s methodology. Also, our model includes census division dummies to absorb region-specific factors (ρ_r), year fixed effects (ϕ_t), and occupation fixed effects (ψ_o). Finally, we cluster our standard errors at CZ-level to address potential serial correlation within an area and weight observations by CZ share of national population in 2019, allowing us to upweight more populated areas while fixing weights at pre-pandemic level.

To shutdown effects driven by changes in employer composition, we run a second specification that restricts our sample to employers that post job ads for a given occupation both before (i.e., 2019) and after the pandemic (i.e., in any of the following years), and evaluate demand changes within a firm-occupation pair. In particular, we define our outcome variables as change in outcome y in firm j occupation o in commuting zone c in year t relative to 2019, and obtain the following specification:

$$y_{j,o,c,t} - y_{j,o,c,2019} = \alpha + \beta_1 \Delta^{t-2019}\text{Tightness}_{c,t} * S_o + \beta_2 \Delta^{t-2019}\text{Tightness}_{c,t} * (1 - S_o) \\ + \boldsymbol{X}_{c,2019} + \rho_r + \phi_t + \eta_{j,o} + \epsilon_{j,o,c,t} \tag{15}$$

To construct our measure of labor market tightness, $\text{Tightness}_{c,t}$, we deviate from the traditional Beveridge curve method of estimating a ratio of vacancy rate to unemployment rate ($\frac{v}{u}$). There are several reasons for this choice, which we elaborate on in Appendix A, but the key point is that the pandemic induced an unusual scale of temporary layoffs, which rendered traditional measures of unemployment imprecise.[21] Therefore, we opt to create a measure of labor market tightness following the spirit of Peri and Zaiour (2023) who defined tightness as the total number of vacancies (from JOLTS) divided by the sum of total employment (from CPS) and vacancies, such that:

$$\text{Tightness}_t = \left(\frac{Vacancy}{Vacancy + Employment} \right)_t \tag{16}$$

[21] In Appendix A, we benchmark our proposed measure of tightness with the measure by Autor et al. (2023), based on the employment-to-employment separation rate and the unemployment rate, and find a high correlation between the two.

We improve on their measure by substituting JOLTS vacancies with Lightcast online job postings and taking advantage of the higher granularity of our data in terms of industry and geography.[22] And to calculate total sum of employment at a more granular level (county), we use LAUS. We then create a measure of *percentage change* in local tightness in commuting zone c between year t and 2019 by taking the difference in logs as follows:

$$\Delta^{t-2019}\text{Tightness}_{c,t} = \ln\left(\frac{Postings}{Postings + Employment}\right)_{c,t} - \ln\left(\frac{Postings}{Postings + Employment}\right)_{c,2019} \tag{17}$$

Notice that the measure of labor market tightness presented in equation 17 is not a function of industry. This is unsatisfactory because industrial level is where we expect most variation in tightness and slack during the pandemic. To add this component, we build a Bartik instrument that predicts changes in labor market tightness by distributing national job postings in 3-digit industry i to each CZ c according to the industry's local employment share in 2017. More precisely, we proceed in the following fashion:

$$\Delta\widehat{Postings}_{c,t} = \sum_i \pi_{i,c,2017} \times \mathscr{P}^{\text{US-wide}}_{i,t-19} \tag{18}$$

where π_{ic} is 3-digit NAICS industry shares in 2017 summing to 1 across industries for each commuting zone c and $\mathscr{P}_i$ is the national industry i shift in job postings for each observed year t relative to 2019. We also compute the leave-one-out version (we refer to it as "LOO"). We finalize our shift-share instrument by standardizing changes in postings by CZ pre-Covid working population and taking the log to obtain:

$$Z_{c,t} \equiv \ln\left(\frac{\Delta\widehat{Postings}_{c,t}}{WorkingAgePop_{c,2017}}\right) \tag{19}$$

Our shift-share instrument has three appeals. First, vacancy count at the local level, especially in small locations, may be measured with some error, whereas our measure allows for more precision. Second, while first differences absorb some of the unobserved variables, actual vacancy growth could potentially reflect location-specific time-varying shocks, which might be problematic if correlated with hiring standards. Third, the use of shares measured years prior to the Covid pandemic to apportion vacancies, along with the unexpected nature of the pandemic, solves the reverse causality issue stemming from location-by-industry employment responding to changes in skills occurring during or due to Covid-19. Using the instrument in equation (19), we then predict

[22] While the concepts of job posting and vacancy are not the same, previous studies have established that Lightcast sample is well-aligned with JOLTS openings (Chetty et al. (2020); Dalton et al. (2020)). See further validation exercises alleviating this concern in Appendix C. The literature has also used job postings to proxy for vacancies. For instance, see Forsythe, Kahn, Lange, and Wiczer (2020).

$\Delta^{t-2019}\text{Tightness}_{ct}$ defined in equation (17) with the following first stage:

$$\Delta^{t-2019}\text{Tightness}_{c,t} = \alpha + \beta_1 Z_{c,t} + \beta_2 S_o + \boldsymbol{X}_{c,2019} + \rho_r + \phi_t + \psi_o + \epsilon_{c,t} \tag{20}$$

To demonstrate how our labor market tightness measure captures geographic heterogeneity, Table 1 lists the top and bottom five commuting zones with the largest and smallest changes in our measure of tightness between 2019 and any of the subsequent three years. To contextualize these changes in terms of size of commuting zones, the last column reports the percentile of CZ population in 2017. While tightness increased and decreased the most in smaller commuting zones, larger CZs also displayed quite large swings, as showed in Panel B where we restrict the exercise to CZs whose population is equal or above the 90^{th} percentile of the population distribution.[23]

5.1 Instrument validity

To assess instrument validity, we discuss the two standard IV assumptions: relevance and exclusion restriction. For relevance, we report in Table 2 estimates from first-stage regressions and show the first-stage F-statistics. Given that our regressions feature a single endogenous regressor, we consider the relative asymptotic bias test for weak instruments, as discussed originally by Olea and Pflueger (2013), more appropriate in presence of clustered errors. Conducting the test at the 5% confidence level, our "effective" F-statistics reported in the table surpass the critical value for 2SLS with a worst-case bias of 10%. Despite the more stringent threshold, if compared to the standard Stock- Yogo critical values for the i.i.d. case, we can confidently reject the null hypothesis of weak instruments.

As expected our instrument is positively and significantly associated with our measure of labor market tightness, regardless of the set of controls included.[24] Our preferred specification for subsequent analysis is Column (6) where we use occupation-level observations, we employ the leave-one-out version of our shift share instrument, and we include the full set of controls, dummies for census divisions regions, year and occupation fixed effects, weighting the sample by CZ share of national population in 2019. As additional check, Table 3 shows the link between our shift-share instrument and the number of postings across CZs, normalized to 2019, and find a positive, significant relationship in all specifications tested.

For the exclusion restriction assumption, it is hard to think of how 2017 local industry shares might directly affect *changes* in skill requirements in 2022 except through the impact of the pandemic on demand for jobs, once we remove remote postings. Plus, the unexpected timing and random nature of the pandemic supports the assumption that employers did not change their location in anticipation of a pandemic. We do not require that the shares predict nothing in levels, but simply

[23]We report another metric of the variation of labor market tightness at the CZ-level also in Figure 17 in Appendix C.

[24]In Table 3 we show that our instrument is positively and significantly associated with the growth of job postings.

that the shares only predict changes through the causal channel emphasized by our design.

As a sanity check, Figure 12 shows the top-5 industries in terms of Rotemberg weights and the relationship between weights and first-stage F-statistic. The five industries driving our 2SLS estimator are: Transit and Ground Passenger Transportation, Educational Services, Ambulatory Health Care Services, Food Services and Drinking Places, and Hospitals, (closely followed by Accommodation).[25] This evidence is reassuring since all these industries are industries that we expect to have experienced a significant change in demand during the pandemic. In Appendix B we provide a broader discussion and additional evidence on the validity of our instrument.

6 Results

We start by considering Table 4, which shows the effect of changes in local labor market tightness on the share of postings listing education/experience estimating equation 14. Odd columns pertain to education, while even columns to experience. Columns 1 and 2 report OLS estimates, columns 3 and 4 report 2SLS estimates using the standard shift-share IV described in Section 5, and columns 5 through 8 report the leave-one-out (LOO) version of our IV. Columns 7 and 8 represent our preferred specification since they include occupation fixed effects allowing us to estimate within occupation effects. Column 1 shows a negative association between tightness and the share of postings listing education, whereas column 2 shows that tightness had no effect on the share of low-wage, low-skill postings listing experience and a negative effect on the share of other postings listing experience. When we introduce our instrument in columns 3 and 4, 2SLS coefficients for both outcomes and groups are now negative and statistically significant. Furthermore, their magnitude increases in absolute value, suggesting that our instrument is addressing the endogeneity biasing OLS estimates. Using the LOO version of our instrument seems to have little effect on sign and magnitude of the estimates, as columns 5 and 6 show. Adding occupation fixed effects in columns 7 and 8 barely changes the share of postings listing education, but increases the share of postings not listing experience by 20% compared to column 6 in the low-wage, low-skill group, while the effect on other postings is virtually unchanged.

Overall, the negative, statistically significant coefficients in the first row indicate that tighter labor markets decrease the probability of job ads listing education and experience requirements for the most vulnerable group of workers in the labor force. Comparing low-wage, low-skill occupations to others, we observe that for education, the negative impact is larger for low-wage, low-skill jobs, while for experience the opposite is true. Since our outcomes are percentage point changes relative to 2019, we can interpret the coefficient on low-wage, low-skill jobs interaction with labor market tightness from columns 7 and 8 to mean that doubling CZ's tightness relative to its 2019 level is predicted to decrease, for this group of jobs, the share of job postings requiring some level

[25] These industries are those with the five highest Rotemberg weights, which captures the variation our instrument is using the most.

of education (experience) by 0.34 (0.11) percentage points. In other words, moving from the 25^{th} percentile to the 75^{th} percentile local tightness leads to a within-occupation decrease in the share of job postings requiring some level of education (experience) by approximately 0.13 (0.04) percentage points.

Looking beyond the share of postings listing requirements, Table 5 examines the effect of labor market tightness on the average years of education and experience listed (intensive margin). Similar to the extensive margin, OLS estimates appear to be attenuated towards zero when compared to 2SLS ones. Considering our preferred specification in columns 7 and 8, we see that while labor market tightness did not significantly affect average years of experience listed once we include occupation fixed effects, it significantly increased the level of education required for low-wage, low-skill jobs. Doubling a CZ's tightness is predicted to increase the required years of education by 0.55 years for postings of a low-wage low-skill occupation, relative to 2019 levels.[26] Interestingly, the main coefficients of interest lose significance in column 8 when we add occupation fixed effects, suggesting that the observed average increase in years of experience listed during tighter markets is driven by variation across occupations rather than within occupation.

We next turn our attention to employers' salary posting behavior to understand whether changes in skill demand translated into changes in advertised salaries. In Table 6 we examine the effect of labor market tightness on the share of postings advertising a salary (extensive margin in odd columns) and the growth rate of posted wages (intensive margin in even columns, after controlling for inflation and taking log). Looking at column 5, we can see that an increase in labor market tightness does not increase the share of employers posting wages. This is true both for low-wage, low-skill as well as other occupations. Looking at column 6, we see that conditional on listing a salary, an increase in local labor market tightness by 10 log points (about 10%) leads to an approximate 2.3% increase in the salary advertised within occupation for low-wage, low-skill jobs relative to 2019. For all other occupations, salary growth is positive but not significant.

To contextualize these results, recall that the US unemployment rate was at a historical low right before the World Health Organization declared a global pandemic in March 2020. In response to their announcement, stay-at-home orders were enacted and labor markets slacked. During this period, many workers reallocated to better-paid jobs and to jobs with a lower risk of contagion. Therefore, when stay-at-home orders were lifted and the economy gradually re-opened, employers in industries heavily reliant on low-wage, low-skill workers faced significant difficulties in filling vacancies due to a sudden demand growth that tightened the labor market. Since those positions required very low levels of education (high school degree) and experience (1.6 years) to start with, a significant number of employers removed any education/experience requirement (Table 4) and, conditional on listing a wage, increased advertised wages.

[26] A 0.55 increase in the education intensive margin is approximately equal to moving from the 25^{th} percentile to the 75^{th} percentile in the distribution of occupation-year-CZ changes for this outcome.

While this seems like a credible story, observed changes could be driven by compositional changes in the set of active employers over time – especially since posting online became more common with stay-at-home orders, and since the pandemic might have driven a non-random subset of firms out of the market. To address this concern, Table 7 and 8 restrict our sample to incumbent employers that we observe posting both before and during the pandemic for a given occupation title. In these regressions, we adopt the model presented in equation 15, where changes in outcomes relative to 2019 are computed at the firm-occupation-commuting zone level, rather than at the occupation-by-commuting zone level. In other words, these estimates capture changes within firm-occupation pairs in each location.

Table 7 presents the effects on the extensive and intensive margin demand of incumbent employers for education and experience. First, for low-wage low-skill jobs, the negative effect of labor market tightness on share of posting listing education reported in Table 4 and positive effect on years of education required in Table 5 are both confirmed in columns 1 and 3. Since both coefficients remain significant (albeit only at the 10% level for the intensive margin) but attenuated toward zero, taking into account compositional changes seems important to remove a confounding effect from our previous estimates. As for the extensive margin, we find that an increase in tightness relative to 2019 that would bring a commuting zone in a given year from the 25^{th} percentile to the 75^{th} percentile in the distribution of observed changes is predicted to decrease the share of job postings requiring some level of education by roughly 0.04 percentage points. This effect is small but non-negligible, considering that the average change for the education extensive margin in the sample is -2.4%, starting from a baseline of 40% for low-wage low-skill jobs (Table 9). As for the intensive margin, doubling CZ's tightness relative to 2019 is predicted to increase average years of education listed by 0.01 years in postings for low-wage, low-skill positions. Since years of education required for low-wage, low-skill positions decline by 0.1% on average in the sample, from an average of 12.4 years (Table 9), this effect is modest. Importantly, however, tightness had no significant effect on the education margins for postings of other jobs (i.e., not part of the low-wage, low-skill group).

Second, considering demand for experience, we see from column 2 of Table 7 that the effect of labor market tightness on share of postings by incumbent employers listing experience is positive and significant for low-wage, low-skill jobs. On the one hand, compared to the coefficient on the other interaction term, this estimate confirms that tightness had a different effect for this specific set of jobs. On the other hand, compared to the very different estimate in column 8 of Table 4, this finding suggests again the presence of compositional changes.Examining the intensive margin for experience, column 4 of Table 7 shows that the coefficient for low-wage, low-skill jobs is positive but insignificant, as it was in Table 5, though once again with a smaller magnitude.

The question, then, is what could explain this observed pattern of demand for education specifically for low-wage, low-skill jobs, where even the same employer, for the same job title, is less likely to post a requirement when facing higher local tightness but, conditional on posting, they demand higher skill levels. One possibility for the negative sign in front of the extensive margin estimate of education for low-wage, low-skill positions is a lower bound faced by employers, since these positions on average require low levels of education and experience (around 12 years of schooling and one to two years of experience, as shown in Table 9). Indeed, demanding fewer years would be nearly equivalent to not specifying any requirement, especially for education. We then hypothesize that the increase in years of education required (and of experience, even though not significant) observed in Table 8 could be explained by employers' willingness to hire some workers in managerial or supervisory roles to manage and mentor the less-qualified new hires.

To explore this possibility, we add to our set of outcomes the share of job postings that require "people management" skills. Following Deming and Kahn (2018), we use the open text field we observe in job ads to categorize a posting as requiring management skills if keywords related to supervision and management are present.[27] Estimates from column 5 of Table 7 support our hypothesis, as employers facing higher levels of local tightness are more likely to demand management skills within the same occupation title. The effect is statistically significant at the 1% level for low-wage, low-skill positions, and almost double in magnitude with respect to other jobs.

Finally, we conclude our analysis by examining the effect of labor market tightness on share of incumbent employers posting wages and the sign and magnitude of their posted wage growth conditional on posting. First, contrary to what emerged from Table 6 for all employers, by considering column 1 of Table 8 we see that the share of incumbent employers posting wages for low-wage, low-skill jobs increased due to tightness (statistically significant estimate at the 10% level). This suggests that, when faced with higher recruiting competition, incumbent firms also respond by posting wages. Second, examining the level of posted wages by incumbent firms, column 2 of Table 8 reports a positive and statistically significant coefficient, confirming the previous finding from Table 6 for all employers, albeit with a smaller magnitude. Within a firm-occupation pair, employers in locations experiencing higher tightness responded by posting higher salary offers for low-wage, low-skill positions as additional recruitment strategy, benefiting new workers in low-wage, low-skill positions.

Converting these gains to dollar values, our estimates in Table 8 imply that an increase in local labor market tightness by 10 log points (roughly 10%) increases salaries advertised for low-wage, low-skill jobs by 1.4% relative to 2019 within the same employer-occupation pair. Since for these positions the average change in salaries posted during the pandemic years relative to salaries posted in 2019 is +4%, this effect is remarkable. Given that the average 2019 salary was approximately 30,700 USD (in 2019 dollars), then an increase in local labor market tightness by 10 log points leads to a 429 USD increase in offered wage (in 2019 dollars), confirming the central role of labor market

[27] Specifically, we require the presence of any of the following keywords: *(supervis.., lead.., manage.., mentor.., staff)*.

tightness in leveling the playing field. Indeed, this positive and statistically significant effect (at the 5% level) is present only for low-wage, low-skill positions, while the coefficient is smaller, negative and not significant for other jobs.

7 Conclusion

In this paper, we examine the effect of labor market tightness on changes in employers' skill demand and advertised salaries by utilizing the heterogeneous effect of Covid-19 across industries and the pre-pandemic variation in industrial composition across commuting zones. Using Lightcast job posting data, we track changes in demand for education and experience, and construct a measure of local labor market tightness and a shift-share instrument. We then employ a long differences model to study changes in our main variables between 2019, and years leading up to and including 2022.

Our analysis show that local labor market tightness affected low-wage, low-skill occupations differently from the rest of the labor market. Increases in local tightness prompted employers to decrease the share of postings listing an education requirement to relax the constraint of not being able to decrease skill requirements further for low-wage, low-skill occupations. However, conditional on listing education, employers increase requirements, along with advertised salaries suggesting that skill demand and wages are complementary levers to attract qualified candidates. These findings support Autor et al. (2023)'s conclusion that pandemic-induced tightness led to wage compression among the most vulnerable workers.

The evidence provided in this paper has important implications for policymakers. First, we shed light on the complementarity of the recruiting strategies adopted by employers and document a demand-side mechanism that explains the wage compression observed during the Covid-19 pandemic. This deeper understanding of how recruitment dynamically and strategically reacts to local conditions, can help explain workers' unemployment duration, job match quality and future wages, and inform policies aimed at improving job search effectiveness, like unemployment insurance, and employment subsidies. Second, our findings confirm that competition and bargaining power dynamics in the labor market determine the distributional effects of aggregate market shocks. Therefore, policymakers should take into account variation in labor market tightness across localities when drafting federal level policies responding to nationwide shocks.

Overall, given the predominant role that tightness had in discussions about the US labor market in the aftermath of the pandemic, and the role it is likely to play in the future, due to retirements, limited internal mobility, and restrictions to immigration, we believe that this line of research deserves careful consideration and further exploration.

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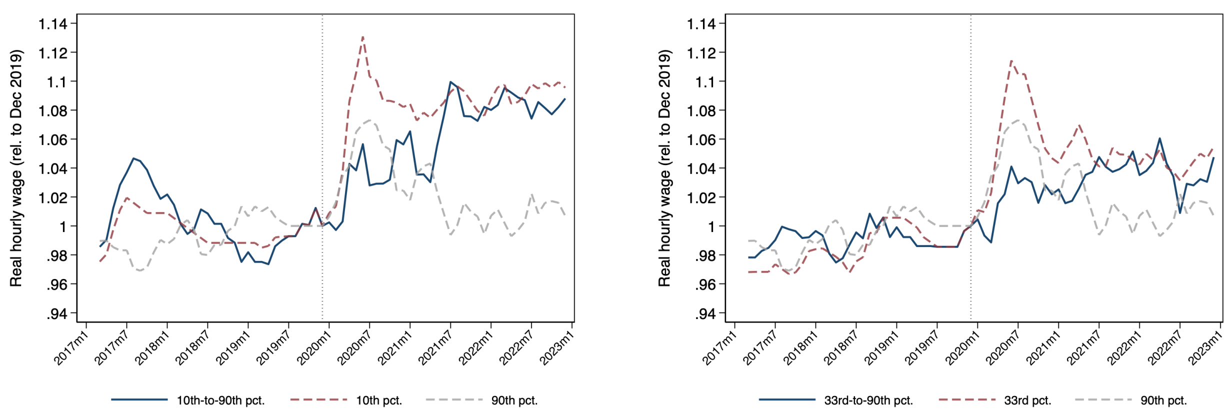
Figure 1: Evolution of US wage inequality before and after Covid-19

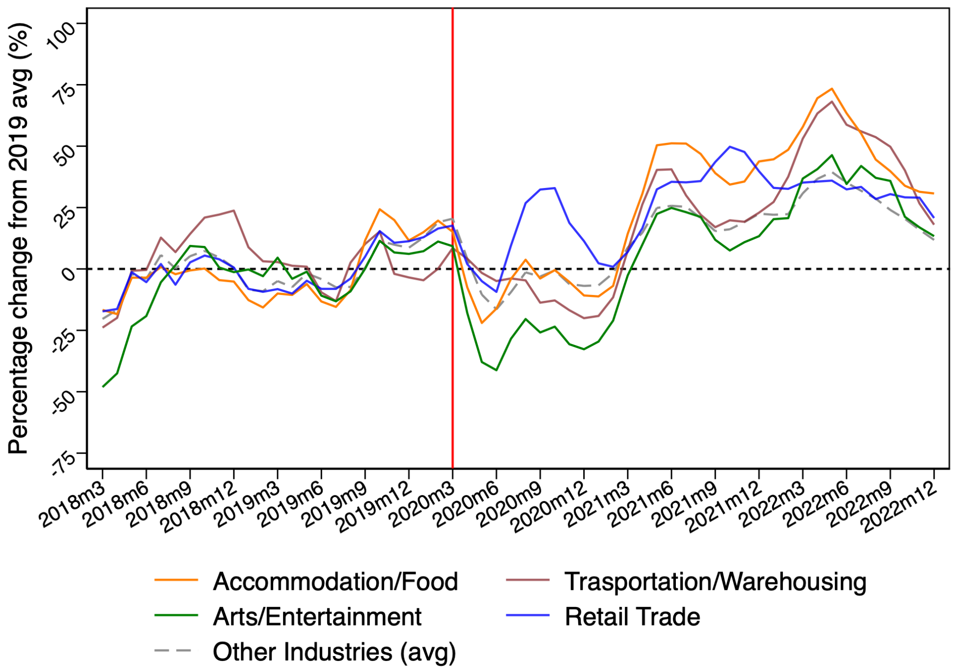
(a) $10^{th} - 90^{th}$ Percentile

(b) $33^{th} - 90^{th}$ Percentile



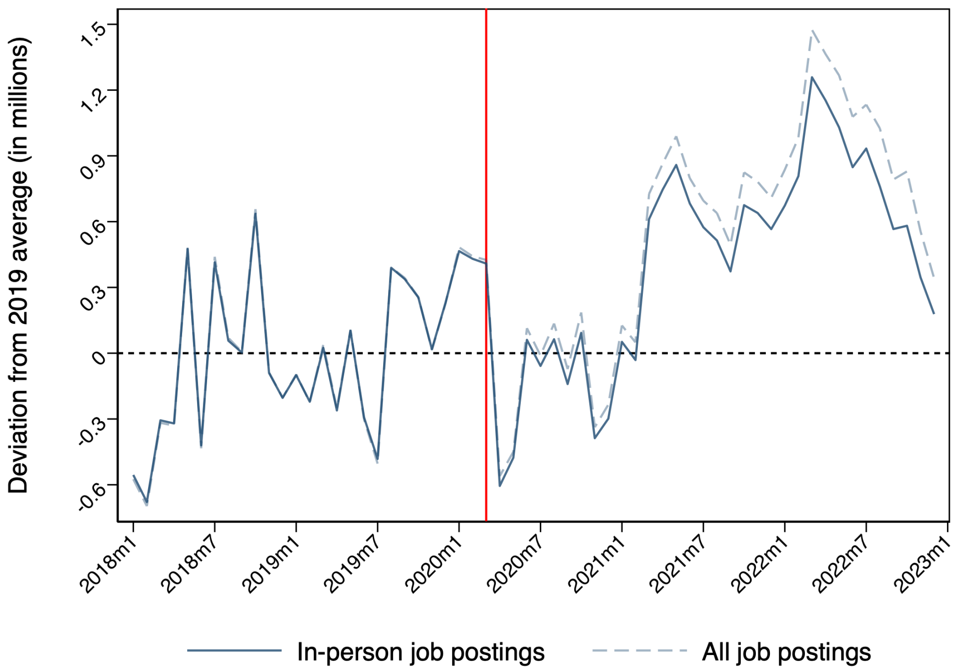
Notes: This figure displays the evolution of wage inequality. We restrict the sample to non-institutionalized working-age (18-65) individuals reporting a positive wage income. Hourly wages have been converted to December 2019 US dollars using the CPI multiplier provided by BLS. Panel A plots the ratio between the 3-month moving averages of monthly 10th percentile wage to the 90th percentile wage (solid navy line). In Panel B, we plot the ratio between the three-month moving averages of monthly 33rd percentile wage to the 90th percentile wage (solid navy line). All series are normalized to one in December 2019.
Source: Monthly CPS, January 2017-December 2022.

Figure 2: Change in volume of job postings by industry



Notes: This figure displays the evolution of online job postings by industry (2-digit NAICS) over time relative to pre-pandemic trends. We display the four industries with the largest deviations during the pandemic and aggregate the others. We restrict the sample to postings that report valid information for industry, occupation and location (county). We drop postings from Alaska and Hawaii. We only keep non-farm, non-military private sector postings. We further restrict to in-person job postings only, aggregating postings at the 2-digit NAICS level and showing percentage changes relative to industry-specific average across 2019 months. We plot three-month moving averages for each industry series.
Source: Lightcast online job postings.

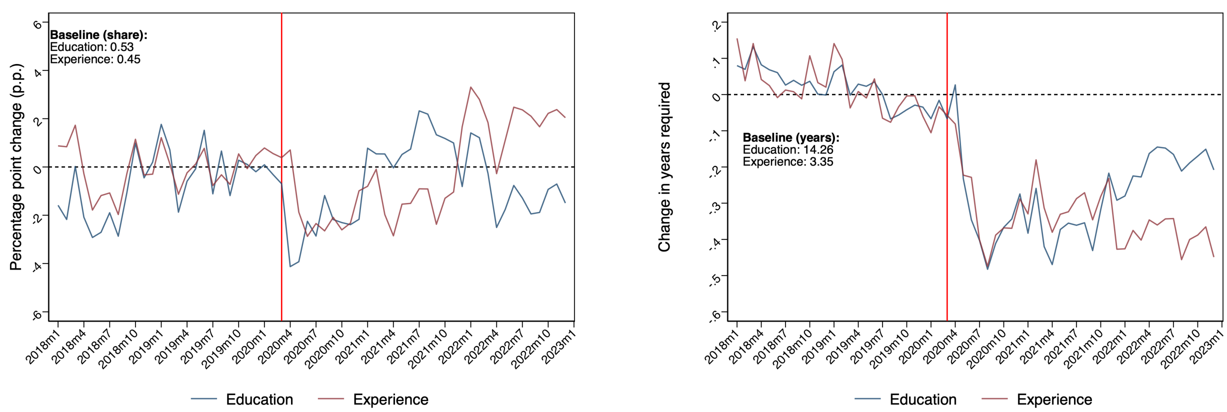
Figure 3: Change in volume of job postings



Notes: This figure displays the evolution of online job postings over time relative to averages across 2019 months. We restrict the sample to postings that report valid information for industry, occupation and location (county). We drop postings from Alaska and Hawaii. We only keep non-farm, non-military private sector postings. In Panel B, we further restrict to in-person job postings only, aggregating postings at the 2-digit NAICS level and showing percentage changes relative to industry-specific average across 2019 months. This will be our sample for the analysis.
Source: Lightcast online job postings.

Figure 4: National evolution of skill requirements in job postings

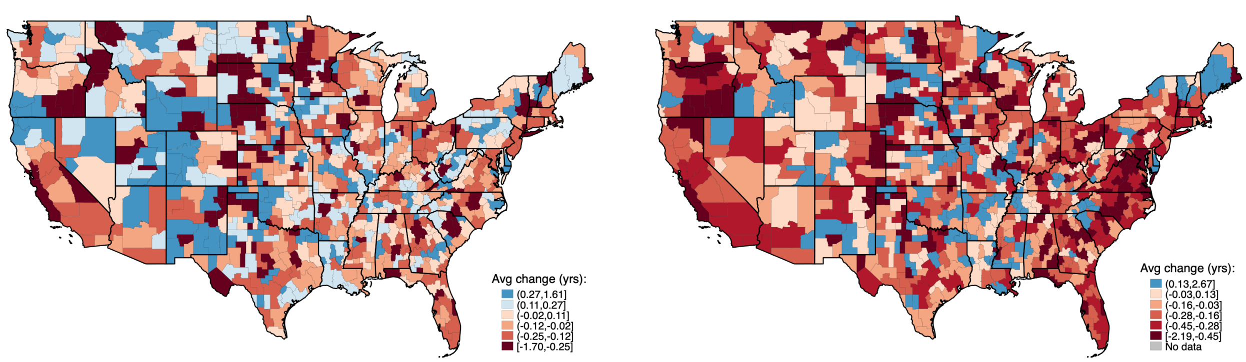
(a) Extensive margin (b) Intensive margin



Notes: This figure displays the evolution of education and experience requirements in online job postings over time. Panel A shows changes in the share of ads posting an education/experience requirement relative to 2019 average across months. Panel B displays changes in average years listed relative to 2019 average across months. In both panels, we restrict the sample to postings with populated information for industry, occupation and location (county). We drop postings from Alaska and Hawaii and only keep non-farming, non-military private sector in-person job postings. This matches our main analysis sample.
Source: Lightcast online job postings.

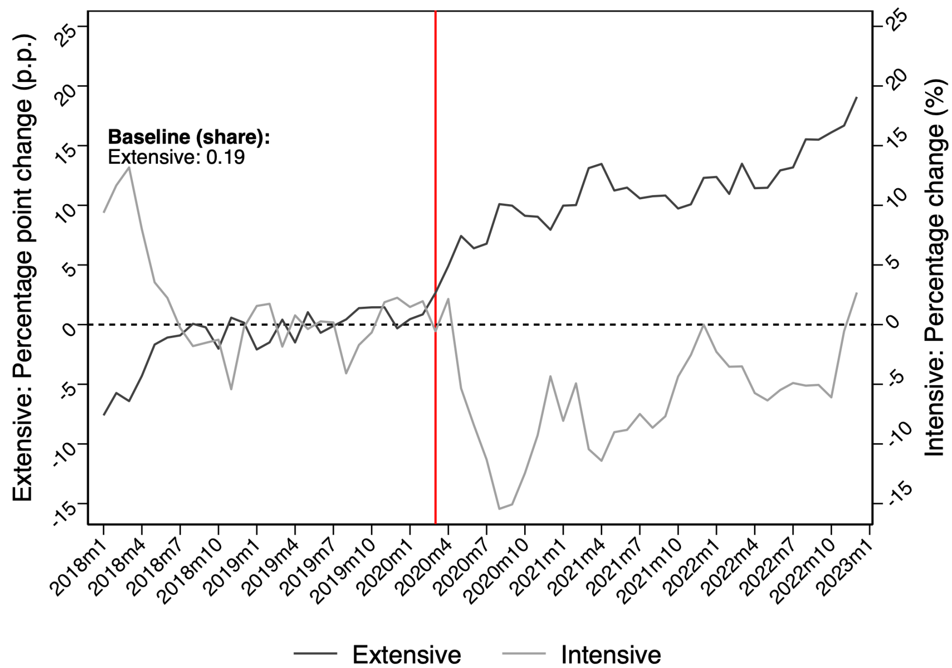
Figure 5: Changes in intensive margin across commuting zones

(a) Education (b) Experience



Notes: This figure displays changes at the commuting zone level in years of education and experience required in online job postings between 2019 and 2022. Usual set of restrictions applied. Changes reported by sextiles of their distribution over commuting zones.
Source: Lightcast online job postings.

Figure 6: National evolution of intensive and extensive margin for posted salary

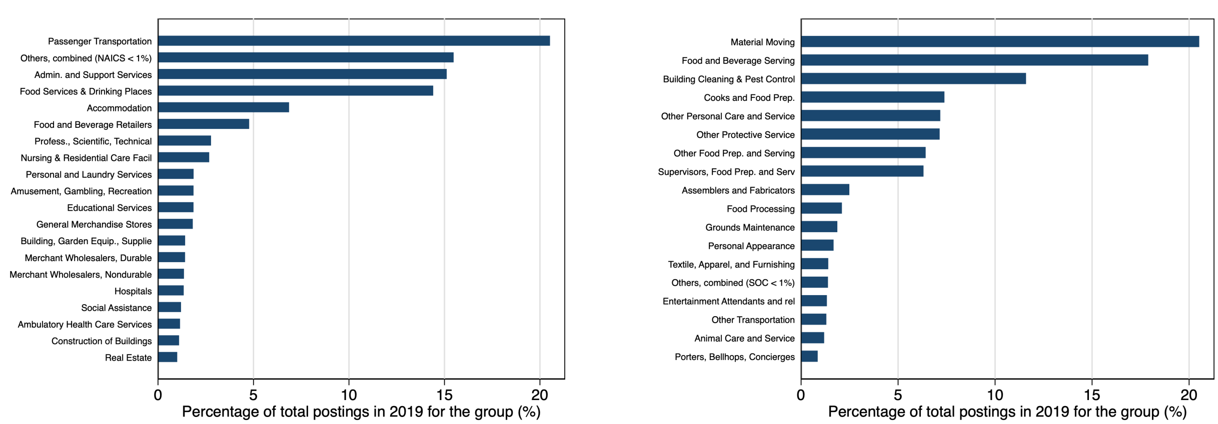


Notes: This figure displays the evolution of the extensive and intensive margins for posted salary over time. It reports both the change in percentage point of the share of postings listing a salary (black line, y-axis on the left) and the percentage change in the level of salaries advertised (gray line, y-axis on the right). Salaries have been converted to December 2019 US dollars using the CPI multiplier provided by BLS. Both changes are computed relative to averages across 2019 months.
Source: Lightcast online job postings.

Figure 7: Composition of job postings for the low-wage low-skill group

(a) Industry

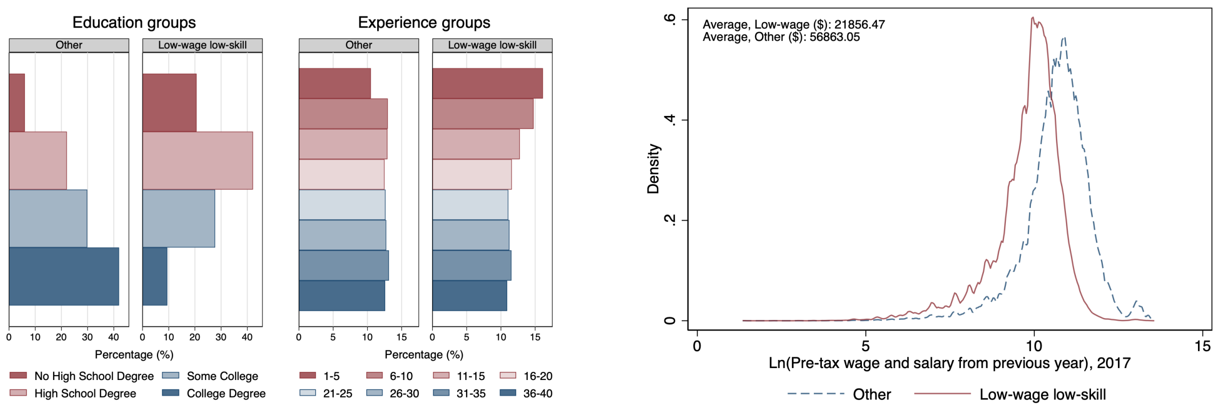
(b) Occupation



Notes: This figure displays the composition of job postings in 2019 for the low-wage low-skill group in terms of industries (3-digit NAICS) and occupations (3-digit SOC). Restrictions to job postings are applied. Low-wage low-skill group includes low-skill service (SOC 35 to 39 and 412) and blue collar occupations (SOC 33 and 45 to 53) in the bottom tercile of the US pre-Covid wage distribution (2015-2019 5-year ACS data).
Source: Lightcast online job postings.

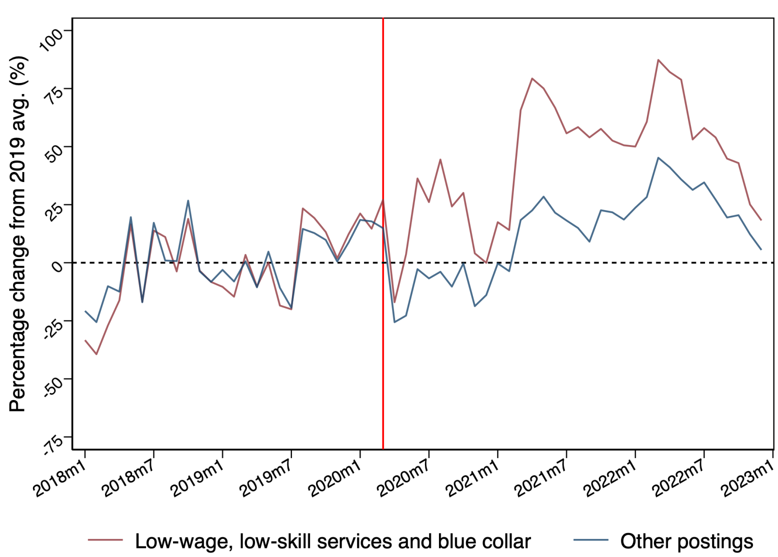
Figure 8: Realized education, experience and wage of low-wage, low-skill workers

(a) Skill (b) Wage



Notes: This figure displays the composition of workers in the low-wage, low-skill group in 2015-2019 in terms of educational attainment (four groups), experience in the labor market (imputed by subtracting years of education from reported age of the individual and divided in eight groups with range of 5-year range), and pre-tax wages (adjusted for inflation to 2017). Sample restricted to working-age (18 to 65 years of age) individuals residing in non group-quarters who are non-enrolled in school. Low-wage, low-skill group includes low-skill service (SOC 35 to 39 and 412) and blue collar occupations (SOC 33 and 45 to 53) in the bottom tercile of the US pre-Covid wage distribution (2015-2019 5-year ACS data).
Source: 2015-2019 5-year ACS data from IPUMS.

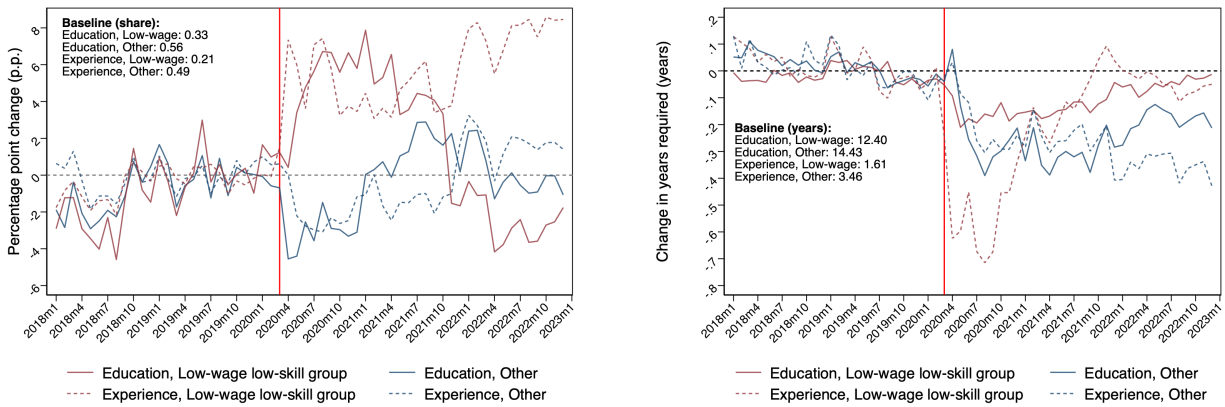
Figure 9: Time series of job postings by group



Notes: This figure displays the evolution of online job postings over time relative to averages across 2019 months by group. We restrict the sample to postings that report valid information for industry, occupation and location (county). We drop postings from Alaska and Hawaii. We only keep non-farm, non-military private sector postings.
Source: Lightcast online job postings.

Figure 10: National evolution of skill requirements in job postings by group

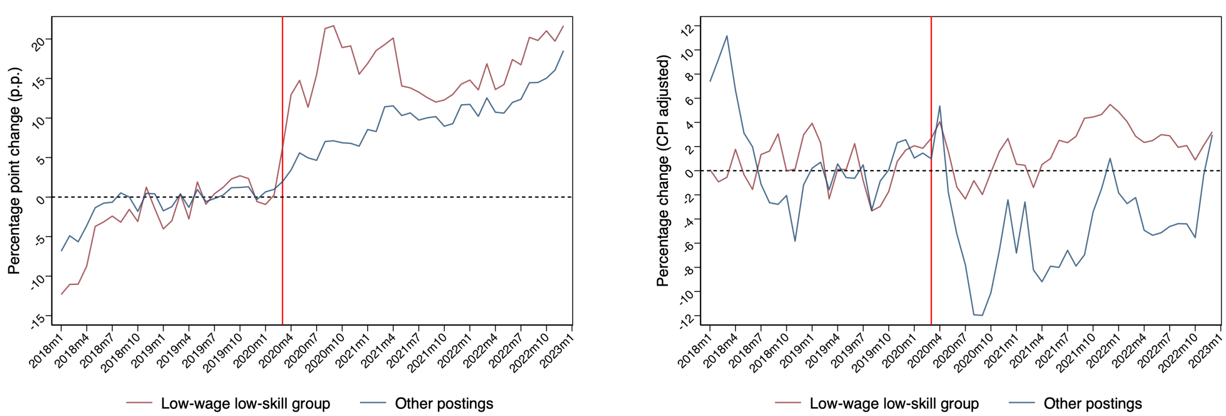
(a) Extensive margin (b) Intensive margin



Notes: This figure displays the evolution of education and experience requirements in online job postings over time by group (low-wage, low-skill vs. all other postings). Panel A shows changes in the share of ads posting a requirement relative to group-specific average across 2019 months. Panel B displays changes in average years required relative to the group-specific average across 2019 months. Underlying data conforms to main analysis sample restrictions.
Source: Lightcast online job postings.

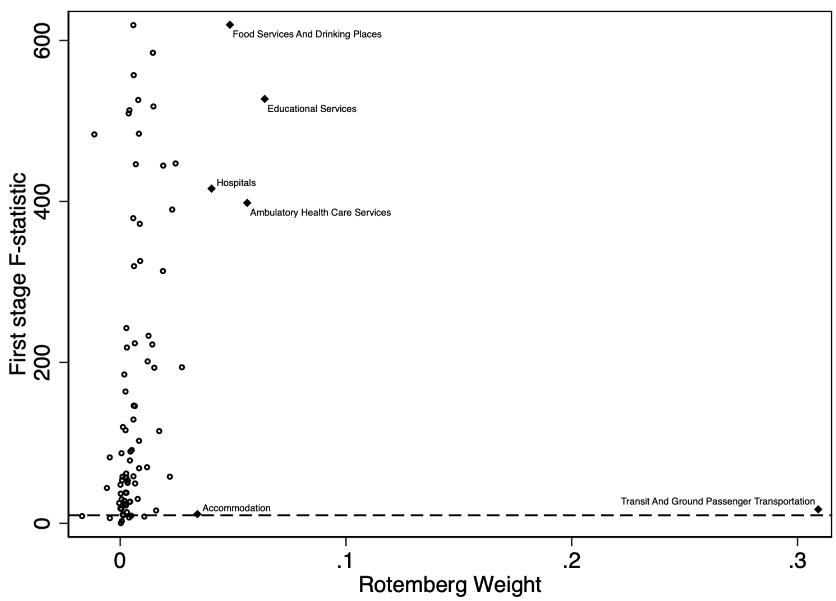
Figure 11: National evolution of intensive and extensive margin for posted salary by group

(a) Extensive margin (b) Intensive margin



Notes: This figure displays the evolution of the extensive and intensive margin for posted salary over time by group (low-wage, low-skill vs. all other postings). Panel A shows changes in the share of ads posting salary information relative to group-specific average across 2019 months, while Panel B displays percentage changes in posted salary (converted to December 2019 US dollars using the CPI multiplier provided by BLS) relative to group-specific average across 2019 months. Underlying data conforms to main analysis sample restrictions.
Source: Lightcast online job postings.

Figure 12: Relationship between the Rotemberg weights and the first-stage F-statistic



Notes: This figure plots each industry-specific instrument's Rotemberg weight against the first stage F-statistic.

Table 1: Changes in measure of labor market tightness by commuting zone-year pair

	CZ code	$\Delta_{t-19} \ln(\frac{Postings}{Postings+Emp})$	Year	Working-age Pop (pct, 2017)
Panel A: Top and Bottom year-CZ pairs				
Sheridan County, MT	26407	-1.22	2022	2
Big Rapids-Ludington, MI	12002	-0.72	2020	33
Bonners Ferry, ID	34501	-0.67	2022	9
Big Rapids-Ludington, MI	12002	-0.60	2022	33
Big Rapids-Ludington, MI	12002	-0.58	2021	33
Garden County, NE	28303	1.23	2020	0
Wichita Falls, TX	32604	1.28	2022	17
Phillipsburg, KS	28603	1.33	2021	6
Phillipsburg, KS	28603	1.50	2022	7
Sheridan County, MT	26407	1.50	2022	2
Panel B: Top and Bottom year-CZ pairs for large CZs				
San Jose, CA	37500	-0.13	2020	96
Glens Falls, NY	18600	-0.12	2020	91
Portland, OR	38801	-0.11	2020	96
New York-Nassau-Suffolk, NY	19400	-0.11	2020	99
Glens Falls, NY	18600	-0.10	2022	91
West Palm Beach, FL	7100	0.46	2020	94
Lancaster-Reading, PA	19100	0.46	2022	92
Hartford-New Haven-Bridgeport-Stamford, CT	20901	0.48	2021	97
Fort Myers-Cape Coral-Naples, FL	7200	0.48	2022	90
San Jose, CA	37500	0.50	2022	96

Notes: Percentiles 25, 50 and 75 of the main statistic are 0.08, 0.23 and 0.40, respectively.

Table 2: Proposed instrument strongly predicts labor market tightness

VARIABLES	(1) Δ LM Tightness	(2) Δ LM Tightness	(3) Δ LM Tightness	(4) Δ LM Tightness	(5) Δ LM Tightness	(6) Δ LM Tightness
Ln Δ Postings (std)	0.0498***	0.0369***	0.0380***	0.0294***		
	(0.00492)	(0.00370)	(0.00541)	(0.00620)		
Ln Δ Posting LOO (std)					0.0282***	0.0285***
					(0.00600)	(0.00578)
Remote Work Share of Pop.		-1.252***	-1.807***	-1.246**	-1.242**	-1.391**
		(0.295)	(0.299)	(0.546)	(0.546)	(0.621)
College-Educated (%)				-0.514***	-0.523***	-0.475**
				(0.175)	(0.174)	(0.186)
Low-wage group			0.657**	0.619**	0.595**	-0.000576***
			(0.268)	(0.263)	(0.265)	(0.000210)
Observations	1,953	1,953	1,933	1,933	1,935	107,969
R-squared	0.326	0.359	0.417	0.438	0.440	0.485
Model	OLS	OLS	OLS	OLS	OLS	OLS
Instrument	Baseline	Baseline	Baseline	Baseline	LOO	LOO
Occ. FE	-	-	-	-	-	Yes
F-stat	102.3	99.51	49.16	22.45	22.08	24.38

Notes: All regressions include census region and time fixed effects. Regressions are weighted by 2019 commuting-zone share of national population. Robust SE in parentheses are clustered at commuting zone. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 3: Proposed instrument and number of job postings

VARIABLES	(1) Postings Growth	(2) Postings Growth	(3) Postings Growth	(4) Postings Growth	(5) Postings Growth
Ln Δ Postings (std)	0.0225***				
	(0.00758)				
Ln Δ Posting LOO (std)		0.0211***	0.0277***	0.0307***	0.0306***
		(0.00744)	(0.00693)	(0.00694)	(0.00695)
Constant	-0.0685	-0.0864	0.0968	0.158	0.0970
	(0.151)	(0.150)	(0.156)	(0.156)	(0.156)
Observations	1,953	1,955	107,969	107,969	107,969
R-squared	0.475	0.480	0.161	0.320	0.360
Model	OLS	OLS	OLS	OLS	OLS
Census Divisions	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No
Occ. FE	-	-	No	Yes	No
Year-Occ. FE	-	-	No	No	Yes
Weights	Yes	Yes	Yes	Yes	Yes

Notes: The outcome variable is the log of the ratio between the number of postings in year t and 2019. Columns 1 and 2 report estimates from regressions at the commuting zone by year level, columns 3 to 5 from regressions at the commuting zone by year by occupation level. Regressions are weighted by 2019 commuting-zone share of national population. Robust SE in parentheses are clustered at commuting zone. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 4: The effect of labor market tightness on share of posting listing education and experience requirements

VARIABLES	(1) Education	(2) Experience	(3) Education	(4) Experience	(5) Education	(6) Experience	(7) Education	(8) Experience
Δ LM Tightness * Low-wage	-0.0613***	0.0145	-0.350***	-0.0860**	-0.347***	-0.0885**	-0.336***	-0.109***
	(0.0124)	(0.0115)	(0.0588)	(0.0369)	(0.0632)	(0.0369)	(0.0575)	(0.0370)
Δ LM Tightness * Other	-0.0294***	-0.0246***	-0.269***	-0.154***	-0.261***	-0.155***	-0.225***	-0.147***
	(0.00926)	(0.00837)	(0.0561)	(0.0362)	(0.0622)	(0.0365)	(0.0573)	(0.0361)
Remote Work Share of Pop.	0.0574	0.0967	-0.231	-0.0672	-0.210	-0.0646	-0.155	-0.0649
	(0.115)	(0.0872)	(0.223)	(0.138)	(0.224)	(0.138)	(0.207)	(0.138)
College-Educated (%)	0.0190	0.00476	-0.179**	-0.0772*	-0.183**	-0.0818*	-0.176***	-0.0813*
	(0.0338)	(0.0285)	(0.0724)	(0.0468)	(0.0728)	(0.0478)	(0.0662)	(0.0474)
Constant	-0.0455*	-0.0389*	-0.0380	-0.0259	-0.0458	-0.0287	-0.0563	-0.0531**
	(0.0242)	(0.0209)	(0.0420)	(0.0264)	(0.0425)	(0.0259)	(0.0393)	(0.0254)
Observations	118,587	118,587	107,780	107,780	107,969	107,969	107,969	107,969
R-squared	0.014	0.038		0.022		0.022	0.109	0.142
Model	OLS	OLS	2SLS	2SLS	2SLS LOO	2SLS LOO	2SLS LOO	2SLS LOO
Census Divisions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occ. FE	No	No	No	No	No	No	Yes	Yes
First-Stage F-stat					24.29	24.29	24.38	24.38

Notes: All regressions are weighted by 2019 commuting-zone share of national population. Robust SE in parentheses are clustered on commuting zone. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 5: The effect of labor market tightness on years of education and experience required

VARIABLES	(1) Education	(2) Experience	(3) Education	(4) Experience	(5) Education	(6) Experience	(7) Education	(8) Experience
Δ LM Tightness * Low-wage	0.128**	0.0212	0.595***	0.410*	0.654***	0.489**	0.551***	0.328
	(0.0556)	(0.0616)	(0.175)	(0.232)	(0.180)	(0.227)	(0.151)	(0.204)
Δ LM Tightness * Other	-0.0480	0.0251	0.150	0.350*	0.236	0.405**	0.223	0.213
	(0.0300)	(0.0439)	(0.168)	(0.205)	(0.151)	(0.206)	(0.146)	(0.191)
Remote Work Share of Pop.	0.167	-0.374	0.491	-0.0675	0.586	0.0223	0.494	-0.205
	(0.361)	(0.465)	(0.474)	(0.604)	(0.490)	(0.618)	(0.484)	(0.566)
College-Educated (%)	-0.276**	0.0413	-0.191	0.290	-0.124	0.324	-0.108	0.246
	(0.125)	(0.162)	(0.157)	(0.243)	(0.163)	(0.246)	(0.159)	(0.226)
Constant	-0.168*	-0.171	-0.183*	-0.216*	-0.181*	-0.227*	-0.258**	-0.172
	(0.0910)	(0.113)	(0.100)	(0.115)	(0.103)	(0.117)	(0.106)	(0.114)
Observations	115,439	111,539	104,948	101,427	105,136	101,615	105,136	101,615
R-squared	0.008	0.009	0.005	0.004	0.003	0.003	0.070	0.061
Model	OLS	OLS	2SLS	2SLS	2SLS LOO	2SLS LOO	2SLS LOO	2SLS LOO
Census Divisions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occ. FE	No	No	No	No	No	No	Yes	Yes
First-Stage F-stat					24.29	24.29	24.38	24.38

Notes: All regressions are weighted by 2019 commuting-zone share of national population. Robust SE in parentheses are clustered on commuting zone. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 6: The effect of labor market tightness on posted salaries

VARIABLES	(1) Salary Ext	(2) Salary Int	(3) Salary Ext	(4) Salary Int	(5) Salary Ext	(6) Salary Int
Δ LM Tightness * Low-wage	0.0649***	0.0666***	-0.148	0.196**	-0.145	0.229***
	(0.0140)	(0.0221)	(0.0944)	(0.0819)	(0.0894)	(0.0733)
Δ LM Tightness * Other	0.0543***	-0.0276	-0.145	0.0691	-0.143	0.0933
	(0.0120)	(0.0171)	(0.0906)	(0.0681)	(0.0886)	(0.0671)
Remote Work Share of Pop.	-0.433**	0.159	-0.661***	0.342	-0.664***	0.370
	(0.186)	(0.218)	(0.254)	(0.247)	(0.248)	(0.249)
College-Educated (%)	0.229***	-0.0236	0.0882	0.0459	0.0960	0.0699
	(0.0702)	(0.0677)	(0.124)	(0.0919)	(0.125)	(0.0928)
Constant	-0.0473	-0.0720*	-0.0542	-0.0705	-0.0498	-0.0677
	(0.0460)	(0.0430)	(0.0581)	(0.0467)	(0.0574)	(0.0473)
Observations	118,587	102,626	107,780	93,157	107,969	93,344
R-squared	0.201	0.098	0.154	0.095	0.156	0.094
Model	OLS	OLS	2SLS	2SLS	2SLS LOO	2SLS LOO
Census Divisions	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occ. FE	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage F-stat					24.38	24.38

Notes: Regressions are weighted by 2019 commuting-zone share of national population. Intensive salary margin in even columns are estimated by taking the natural logarithm of salary level. Robust SE in parentheses are clustered on commuting zone. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 7: Incumbent employers: The effect of labor market tightness on education and experience margins

VARIABLES	(1) Education	(2) Experience	(3) Education	(4) Experience	(5) Management
	Extensive		Intensive		Ext
Δ LM Tightness * Low-wage	-0.0952***	0.0810**	0.0117*	0.120	0.0674***
	(0.0342)	(0.0391)	(0.00647)	(0.0753)	(0.0165)
Δ LM Tightness * Other	-0.0154	-0.0707	-0.00609	0.165**	0.0338*
	(0.0377)	(0.0448)	(0.00647)	(0.0753)	(0.0186)
Remote Work Share of Pop.	-0.00834	-0.0533	0.00277	0.185	0.0433
	(0.0779)	(0.104)	(0.0152)	(0.222)	(0.0574)
College-Educated (%)	-0.0434	-0.0745**	-0.00494	0.124*	0.0170
	(0.0271)	(0.0376)	(0.00506)	(0.0716)	(0.0218)
Mean Y(Low-wage)	0.0240	0.0430	-0.00100	-0.0320	0.00900
Mean Y(Other)	0.0190	0.0250	-0.00300	-0.00900	0.0130
Observations	4,799,605	4,799,605	2,830,811	2,245,004	4,799,605
Model	2SLS LOO	2SLS LOO	2SLS LOO	2SLS LOO	2SLS LOO
Census Divisions	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm-Occ. FE	Yes	Yes	Yes	Yes	Yes

Notes: Regressions are weighted by 2019 commuting-zone share of national population. Robust SE in parentheses are clustered on commuting zone. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 8: Incumbent employers: The effect of labor market tightness on salary margins

VARIABLES	(1) Ext Salary	(2) Ln(Salary)
Δ LM Tightness * Low-wage	0.188*	0.143**
	(0.109)	(0.0610)
Δ LM Tightness * Other	0.0599	-0.0474
	(0.108)	(0.0647)
Remote Work Share of Pop.	-0.665	-0.0889
	(0.500)	(0.150)
College-Educated (%)	0.304	-0.0235
	(0.188)	(0.0682)
Mean Y(Low-wage)	0.107	0.0440
Mean Y(Other)	0.0840	0.0120
Observations	4,799,605	1,050,328
Model	2SLS LOO	2SLS LOO
Census Divisions	Yes	Yes
Year FE	Yes	Yes
Firm-Occ. FE	Yes	Yes

Notes: Regressions are weighted by 2019 commuting-zone share of national population. Intensive salary margin is computed by taking the natural logarithm of (real) salary level. Robust SE in parentheses are clustered on commuting zone. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 9: Outcome averages in the restricted employer sample

	Education (share) (1)	Experience (share) (2)	Education (years) (3)	Experience (years) (4)	Salary (share) (5)	Salary (USD) (6)	Management (share) (7)
Low-wage							
Obs.	1,300,018	1,300,018	672,263	516,708	1,300,018	615,873	1,300,018
Mean	0.40	0.27	12.37	1.59	0.35	30862.79	0.32
Mean (2019 only)	0.38	0.24	12.41	1.75	0.29	30684.03	0.31
Other							
Obs.	7,941,668	7,941,668	5,680,298	5,133,967	7,941,668	2,782,224	7,941,668
Mean	0.60	0.51	14.11	3.23	0.25	52768.42	0.58
Mean (2019 only)	0.58	0.49	14.19	3.31	0.20	53205.10	0.57

A Data and Measures

A.1 Industry crosswalk

In order to take advantage of industry-level information from both ACS and Lightcast sources, we map industry information of Census code contained in ACS data to the North American Industry Classification System (NAICS) code used by Lightcast by using publicly-provided crosswalks.[28] For situations of one-to-many mapping we choose to employ equal weights. Our analysis relies on industries at the 3-digit NAICS level, giving us 86 different industries.

A.2 Tightness measures

As described in Section 5, we build a measure of labor market tightness that does not rely on unemployment for two main reasons. First, standard measures involving unemployment rate would greatly underestimate tightness during some phases of the pandemic due to the explosion of temporary-layoff unemployment. Many workers that were laid off expected to be recalled by their previous employers so they did not engage in usual search behaviors, making the distinction between temporary-layoff unemployment and jobless unemployment extremely important (Hall and Kudlyak, 2022). Second, we devise an empirical strategy that relies on industry heterogeneity to measure local tightness and, if we wanted to use unemployment, it would be hard to find a conceptual link between unemployment and specific industries as, once workers become unemployed, they likely apply to a broad range of jobs in different industries.

Therefore, we opt to build a measure that uses vacancies, which we proxy with job postings, and can take advantage of the granularity of Lightcast data. While vacancies measured by the Job Openings and Labor Turnover Survey (JOLTS) of the US Bureau of Labor Statistics are only available disaggregated by 17 mutually exclusive broad sectors of the economy, which would greatly limit the granularity of the analysis, we can use 86 industries, after aggregating 6-digit NAICS into 3-digit NAICS industries.[29] Importantly, we benchmark our methodology of capturing changes in tightness with changes in the measure proposed by Autor et al. (2023) in Figure 13. While we can construct our measure at the commuting zone level, we bring it to the state level in order to compare the two. Reassuringly enough, our measure built on local online job postings and employment tracks particularly well their measure, which is based on state-level employment-to-employment separation rate from LEHD/J2J data paired with the state-level unemployment rate from LAUS.

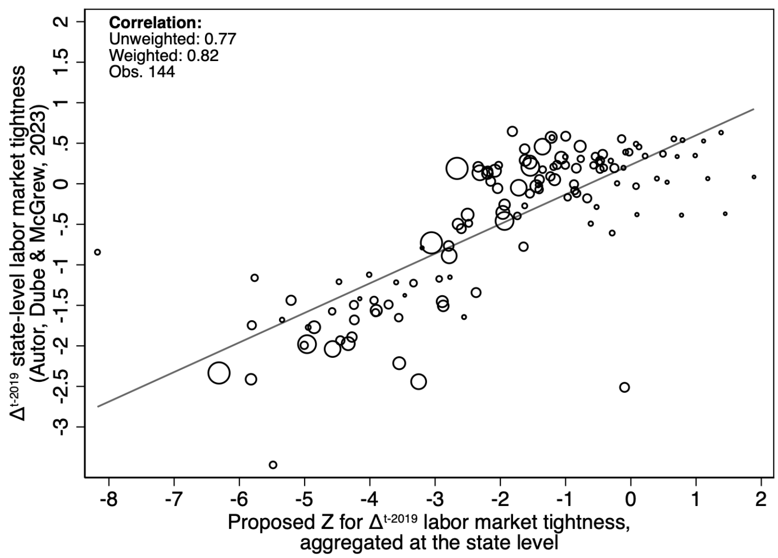
Alternative measures of tightness, mostly available at the national level only, are computed by online job boards such as LinkedIn and Indeed. These platforms exploit the fact that they can observe applicants to job postings to claim they have a more precise measure of job search activ-

[28] `https://www.census.gov/topics/employment/industry-occupation/guidance/code-lists.html`.

[29] In the JOLTS such disaggregation by sector is unavailable at the state level, or at any smaller geographical level, further limiting the scope of the analysis in geographical terms. On the contrary, we observe the county code for each job posting, allowing us to build our measure at the commuting zone level.

ity. Unfortunately, we do not observe "clicks" or applications made per posting in Lightcast data. However, Lightcast is able to capture the almost entirety of openings for the US, providing us with details on crucial dimensions of postings, and we highly value this advantage. At the same time, there is no guarantee that application activity on a platform captures job search accurately as users are likely to be only a subset of the whole population of job seekers.

Figure 13: Changes in Autor et al. (2023)'s labor market tightness vs. our proposed instrument



Notes: This figure benchmarks changes in Autor et al. (2023)'s proposed measure of labor market tightness to our instrument for local tightness. While our instrument captures changes in yearly tightness at the commuting zone (CZ) level from online postings, their measure is built as the average of state-level employment-to-employment separation rate and negative unemployment rate (both standardized), and it is available biannually at the state level. To make the two measures comparable, we start by aggregating ours to the state level using the crosswalk provided by Autor and Dorn (2013) and weighting by the 2019 population of each CZ. Then, we take the average of the two 6-month levels of their measure for each year and state. Since our instrument captures changes in tightness relative to 2019, we also take the difference of their annualized measure in 2020, 2021 and 2022 with its 2019 level. As usual, we exclude Alaska and Hawaii. Changes in tightness are not available for Arkansas, Mississipppi, and Tennesse, and as a result, these states do not appear in the graph. Each point represents a state-by-year observation. Size of the marker represents the state's share of national population in 2019. We report weighted and unweighted correlation coefficients.
Source: Calculations by Autor et al. (2023), Lightcast, LAUS.

B Validity of the shift-share instrument

This section aims at strengthening the credibility of our empirical design by conducting some of the validity tests prescribed by Goldsmith-Pinkham et al. (2020). The Bartik estimator can be decomposed into a weighted sum of the just-identified instrumental variable estimators that use each industry share (π_{jk}) as a separate instrument. The weights, called Rotemberg weights, can be interpreted as sensitivity-to-misspecification parameters, and tell us how sensitive the over-identified estimate of our coefficient of interest is to misspecification (i.e., endogeneity) in any instrument. In other words, they describe the research design: by telling us which exposure design gets more weight in the overall estimate, and thus which of these identifying assumptions is most worth testing, they make clear which variation in the data the estimator is using. In this setting, important comparisons are across places with greater and smaller shares of top 5 industries according to the weights. Hence, testing the specification validity for the sub-sample of industry-specific shares that affect the most the overall 2SLS estimation will reassure on the general validity of our estimations.

We start by computing the Rotemberg weights for each industry to identify the top 5 industries in terms of weights. Panel C in Table 10 lists these industries: *Transit And Ground Passenger Transportation*, *Educational Services*, *Ambulatory Health Care Service*, *Food Service and Drinking Places*, and *Hospitals*. The top 5 instruments together receive almost exactly half of the absolute weight of the estimator (0.518/1.044). Since these instruments refer to the industries that should motivate the empirical strategy, we are reassured to see that they are well-aligned with our expectations. According to our narrative, in these industries, firms should have difficulty in hiring and filling their vacancies during and after the pandemic due to declines or surges of demand. It should be plausible that a labor shortage was the main shock hitting the industry rather than, for instance, simultaneous technological innovations increasing demand (and ultimately affecting changes in skills). It would have been particularly problematic if, for instance, industries prone to secular technological changes had received large weights (as in the case of Autor, Dorn, and Hanson (2013)). In addition to that, we can see in Panel A that the share of industries with negative weights is pretty low.

Importantly, Panel B shows that the national growth rates g_k provide a pretty reliable guide to which industries drive estimates, as the shocks g_k explain an important portion of the variance of the Rotemberg weights (see the correlation between α_k and g_k). Panel B also highlights that weights are not very related to the variation in industry shares across locations ($var(z)$). This does not come as a surprise since our top 5 are not examples of tradables, which by definition have industry shares that vary a lot across locations.

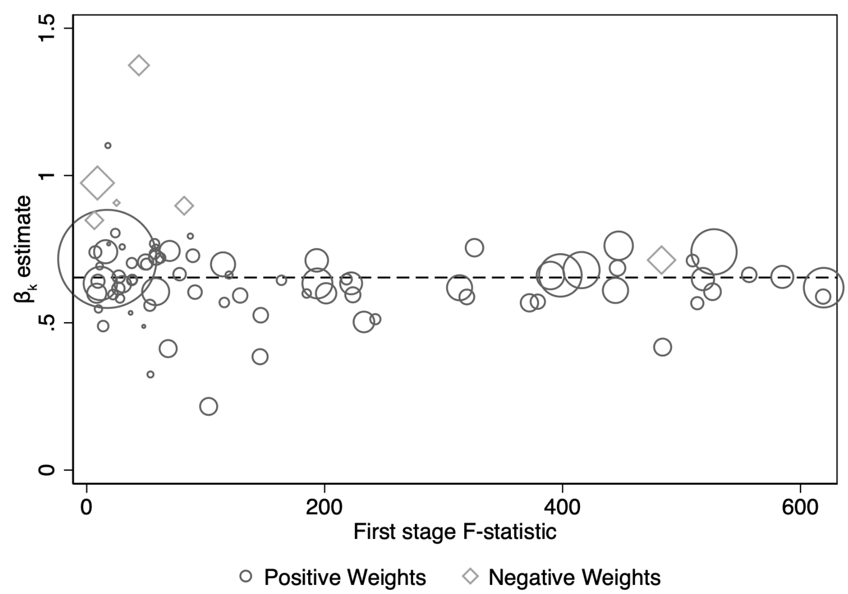
We conclude by showing the relationship between each industry-specific IV's β_i and the first-stage F-statistic in Figure 14. The dispersion in the point estimates among the high-powered industries is low and the high-weight industries appear to be clustered closely to the overall point estimate. While there are negative Rotemberg weights, these industries are few and only small share of the overall weight.

Table 10: Summary of Rotemberg weights

Panel A: Negative and positive weights					
	Sum	Mean	Share		
Negative	-0.044	-0.007	0.040		
Positive	1.044	0.013	0.960		
Panel B: Correlations of Industry Aggregates					
	α_k	g_k	β_k	F_k	$\mathrm{Var}(z_k)$
α_k	1				
g_k	0.689	1			
β_k	-0.002	0.133	1		
F_k	0.079	0.224	-0.125	1	
$\mathrm{Var}(z_k)$	0.185	0.129	-0.029	0.002	1
Panel C: Variation across years in α_k					
	Sum	Mean			
2020	0.061	0.001			
2021	0.256	0.003			
2022	0.683	0.008			
Panel D: Top 5 Rotemberg weight industries					
	$\hat{\alpha}_k$	g_k	$\hat{\beta}_k$	95 % CI	
Transit And Ground Passenger Transportation	0.309	1.34e+06	0.717	(0.700,0.900)	0.610
Educational Services	0.064	3.67e+05	0.741	(0.700,0.700)	9.787
Ambulatory Health Care Services	0.056	8.61e+05	0.661	(0.600,0.600)	5.324
Food Services And Drinking Places	0.049	7.69e+05	0.619	(0.600,0.600)	6.237
Hospitals	0.040	3.92e+05	0.679	(0.600,0.700)	5.375
Panel E: Estimates of β_k for positive and negative weights					
	α-weighted Sum	Share of overall β	Mean		
Negative	-0.041	-0.063	0.952		
Positive	0.695	1.063	0.632		

Notes: This table reports results from the Rotemberg decomposition. We use the change in the (log of) posted salaries, i.e. intensive margin, as outcome of interest.

Figure 14: Relationship between each industry-specific IV's β_k and first stage F-stat



Notes: This figure shows the relationship between each industry-specific IV's β_k and first stage F-stat. Each point is a separate instrument's estimates (industry share). The figure plots the estimated β_k for each instrument on the y-axis and the estimated first-stage F-statistic on the x-axis. The size of the points areflects the magnitude of the Rotemberg weights, with the circles denoting positive Rotemberg weights and the diamonds denoting negative weights. The horizontal dashed line is plotted at the value of the overall $\hat{\beta}$. Only instruments with F above 5 are reported.

C Complementary evidence

C.1 Other estimates on skill demand trends

In Table 11, we validate the main results of Forsythe et al. (2022) by reporting estimates from regressions of posting-level skill requirements on indicators for Covid-19 waves and interactions with postings for low-skill service occupations. In these regressions, "Covid 1" equals one if the date of the postings is between March 2020 and March 2021, and "Covid 2" equals one if the date is between April 2021 and June 2022. "Service" takes value one if the posting belongs to low-skill service occupations (SOC 35–39, 412), and "Low-wage" equals one if posting belongs to our previously defined low-wage, low-skill set of occupations. Like these authors, we include sector-by-occupation-by month fixed effects.

While the data we have currently available only span 2018 to 2022, Columns 1 to 4 replicate closely results in Table 2 by Forsythe et al. (2022), who run the same specification using 2015 to mid-2022 data. In particular, we reproduce well the evidence of downskilling (intensive margin) during both phases of the pandemic ("Covid 1" and "Covid 2" coefficients in Columns 2 and 4) as well as the increased likelihood of posting a requirement in the second phase of the pandemic ("Covid 2" in Columns 1 and 3) documented in their findings. In other words, US national trends show that employers are more likely to ask for a skill requirement during the second wave of the pandemic for a similar type of job but, conditional on asking, they request fewer years of both education and experience. This evidence also aligns with the evolution of national skill demand displayed in our Figure 4. Looking at the interaction terms, our estimates in Table 11 also suggest that postings for low-skill service occupations, while becoming slightly less likely to report a requirement by the time the second Covid-19 wave hit, exhibit a larger decline for years of experience required, exactly as in Forsythe et al. (2022).

In Column 5 to 8 we then run the same specifications but substitute, in the interaction terms, Forsythe et al. (2022)'s category of low-skill service jobs with our group of low-wage, low-skill service and blue collar jobs that we use in our main analysis. While the two groups differ, Column 6 and 8 show that postings for our group of interest show a downskilling trend very similar to the one observed for theirs.

All in all, while we are reassured by our ability to replicate their main descriptive results, we stress the fact that there is no reason for our main analysis on skill demand to mirror these trends. On top of differences in sample years and restriction decisions on postings (e.g., we strictly require postings in our final sample to have precise employer, county, industry and occupation information), we focus on isolating the local causal impact of tightness, rather than describing national dynamics.

C.2 More on job postings and tightness heterogeneity

While we observe a great degree of heterogeneity across locations, it is hard to comment on the correlation between the changes in education and experience from previous figures. Hence, in Figure 16, we plot the correlation between the changes in the two intensive margins for the 722 commuting zones of the contiguous US. 46% of commuting zones display a decline in both and are located in the third quadrant. The correlation coefficient between the changes is 0.17 but it goes up to 0.24 once we weight by the 2019 population of each commuting zone. As a benchmark for interpreting this heterogeneity, national average changes in the intensive margins between 2019 and 2022 are reported.

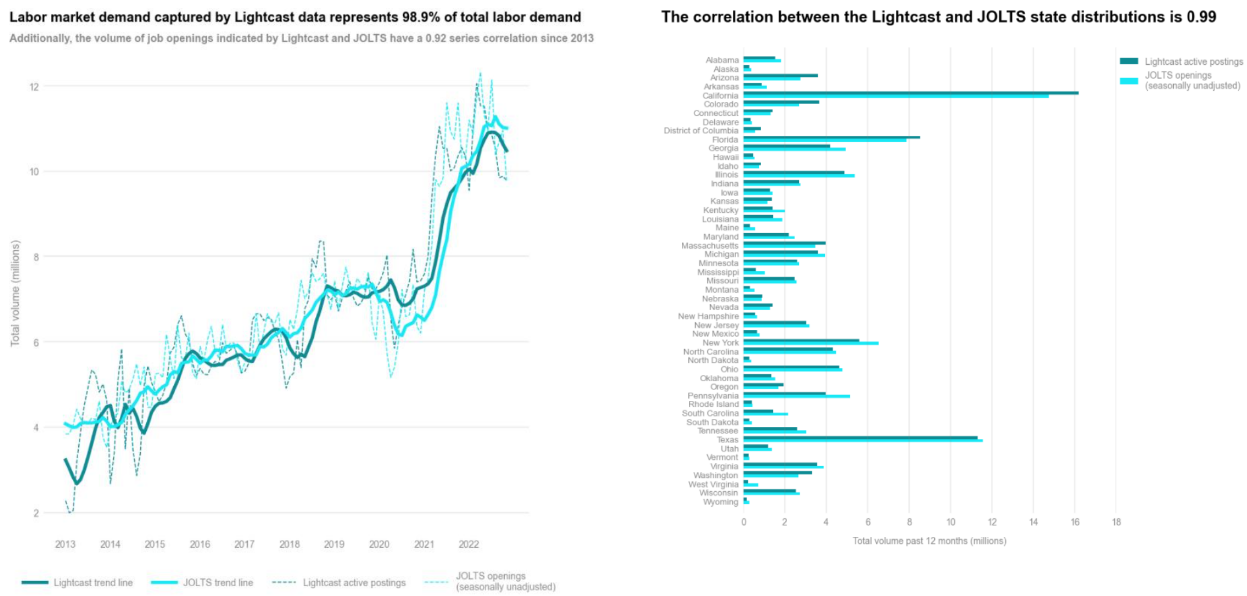
Table 11: Skill requirements in job postings and Covid indicators

VARIABLES	(1) Education Extensive	(2) Education Intensive	(3) Experience Extensive	(4) Experience Intensive	(5) Education Extensive	(6) Education Intensive	(7) Experience Extensive	(8) Experience Intensive
Covid 1	0.00546	-0.127***	-0.00402*	-0.0207*	-0.000323	-0.115***	0.00228	-0.00532
	(0.00329)	(0.0105)	(0.00215)	(0.0109)	(0.00330)	(0.0102)	(0.00229)	(0.0111)
Covid 1 * Service	0.0186***	0.0348***	0.0243***	-0.148***				
	(0.00407)	(0.00795)	(0.00567)	(0.0201)				
Covid 1 * Low-wage					0.0613***	-0.0810***	-0.0120*	-0.468***
					(0.00340)	(0.0184)	(0.00651)	(0.0363)
Covid 2	0.0261***	-0.152***	0.0281***	-0.109***	0.0226***	-0.137***	0.0260***	-0.102***
	(0.00256)	(0.00783)	(0.00308)	(0.00915)	(0.00241)	(0.00756)	(0.00334)	(0.00920)
Covid 2 * Service	-0.0157***	0.116***	-0.0249***	-0.120***				
	(0.00430)	(0.00594)	(0.00543)	(0.0166)				
Covid 2 * Low-wage					0.00797	-0.0297*	-0.0140**	-0.296***
					(0.00515)	(0.0162)	(0.00614)	(0.0376)
Constant	0.515***	14.23***	0.446***	3.287***	0.515***	14.23***	0.446***	3.290***
	(0.00154)	(0.00506)	(0.00137)	(0.00656)	(0.00153)	(0.00512)	(0.00138)	(0.00671)
Observations	150,645,678	79,195,070	150,645,678	68,450,302	150,645,678	79,195,070	150,645,678	68,450,302
R-squared	0.136	0.385	0.107	0.271	0.136	0.385	0.107	0.271
Sector-Occ.-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from OlS regressions of posting-level skill requirements on indicators for Covid waves ("Covid 1" equals one if the date is between March 2020 and March 2021, while "Covid 2" equals one if the date is between April 2021 and June 2022). "Service" is an indicator for postings belonging to low-skill service occupations as defined in Forsythe et al. (2022). "Low-wage" is an indicator for postings belonging to our low-wage, low-skill group as defined above. Robust standard errors are clustered at the year-month level. All models include sector-by-occupation-by month fixed effects (two-digit NAICS and two-digit SOC level) and span 2018 to 2022 period. Columns 1-4 validate the main regression results from Forsythe et al. (2022) (unlike their regressions, employing Lightcast job postings data starting from 2015, our period spans 2018 to 2022).

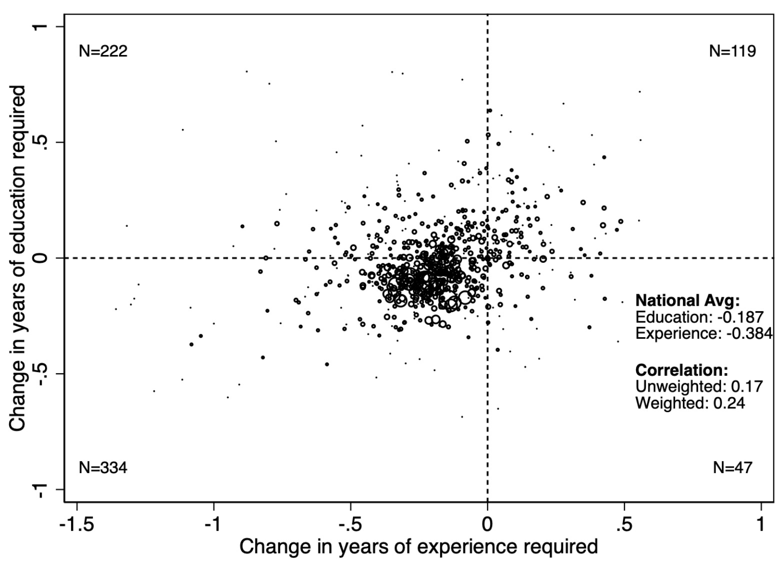
Figure 15: Representativeness of Lightcast sample of online job postings

(a) Time (b) Geography



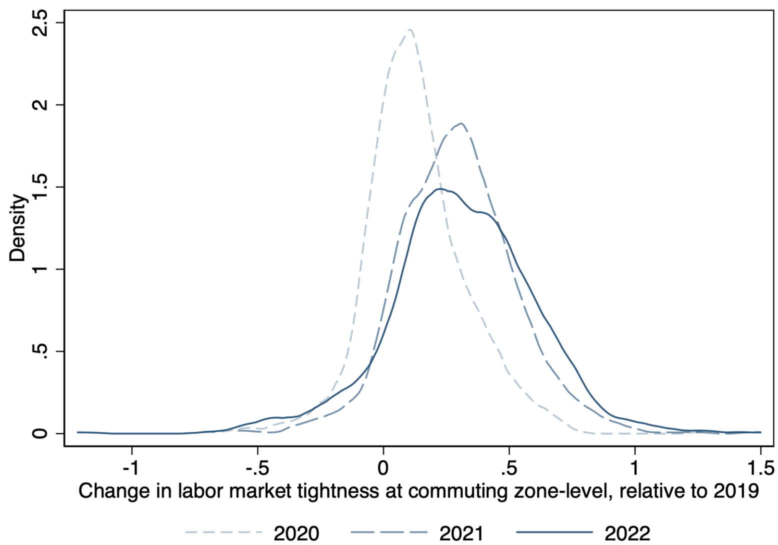
Source: Lightcast.

Figure 16: Correlation between 2019-2022 changes in intensive margins by commuting zone



Notes: This figure shows the relationship between 2019-2022 changes in the average intensive margins (years of education on the y-axis, years of experience on the x-axis) by commuting zone. Each point represents a commuting zone, and the size of the marker represents 2019 population. Commuting zones with changes below the 1 percentile and above the 99 percentile of the distribution of changes are not displayed.
Source: Lightcast online job postings.

Figure 17: Labor market tightness over time



Notes: This figure displays the probability density estimation of the change in labor market tightness relative to 2019 across commuting zones, our main regressor described in equation (17). We plot the density separately for the 2019-2020 (short dashed light blue line), the 2019-2021 (dashed blue line), and the 2019-2022 (solid dark blue line) changes.
Source: Lightcast, LAUS.